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EDITORIAL NOTE.

The GEOLOGICAL MAGAZINE has suffered a heavy loss by the death of Professor J. E. Marr, Sc.D., F.R.S., on 1st October. The Editor began his geological career by attending Marr's lectures in the dark hole under the University Library at Cambridge, so well remembered by many senior geologists, and ever since that time he has been closely associated with the late Professor as pupil, colleague, and friend. Even after his retirement from the Woodwardian Chair of Geology, some three years ago, Professor Marr continued to give the advantage of his advice in the conduct of the Magazine, and the Editor desires to record his deep sense of personal bereavement. It is unnecessary to speak here of Professor Marr as a geologist: his achievements will be set forth in many formal obituaries: it is desired only to emphasize his intimate relations to the GEOLOGICAL MAGAZINE and its Editor.

ORIGINAL ARTICLES.

On the Preparation of Geological Manuscripts.

By THE EDITOR.

NO one who has not had personal experience of it can imagine the amount of labour thrown on the editor of a scientific periodical by the variation in the methods used by authors to indicate special kinds of type, and indeed, if the truth must be told, by the mistakes made by many authors as to what kind of type should be used. Quite often it is necessary, before a manuscript goes to the printers, to alter nearly every indication given as to the use of italics, capital headings, and so on, simply because the wrong signs are employed.

Now here there are two rather different questions involved. Firstly, there is the problem of inducing authors to conform to the standards adopted by any one particular publication; and secondly, the much larger and more difficult subject of the possibility of working out a uniform practice which might in time be adopted by all publications dealing with one branch of science. This latter is undoubtedly a large order, but it does seem worth while to consider it from the point of view of geological journalism, because at present nearly every journal has a different standard and the result is chaos.

If I may be permitted to adopt a somewhat personal note, I would say that, having now edited the GEOLOGICAL MAGAZINE for fourteen years, I may justly claim to have had some practical experience in this matter, and as a result of this experience I have come to very definite conclusions

This subject may be looked at from several different points of view. There is first of all the question of conformity with the practice in other departments of science. For example, it is obviously desirable that names and descriptions of fossils should be treated in the same way as the nomenclature of zoology and botany. In this connection it is only fair to say that palaeontologists do seem to know their job, and there is seldom any serious trouble in this respect, so far as concerns generic and specific names and the quotation of authorities for such names. The language of palaeontology is a jargon, but at any rate it is a consistent jargon.

The second point of view here suggested is that of simplicity: at a later stage some suggestions will be made on this point, for the most part of a purely typographical nature, and the subject will not for the moment be pursued further.

Thirdly, there is the sordid but very necessary consideration of expense in printing. It is probably quite unknown to most authors of geological papers that any departure from the normal type of the journal, such as formulae and tabular matter, not to mention special type, involves an addition to the cost of printing.

After all this introduction, let us get down to brass tacks and consider in detail some of the typographical points that arise. First of all, as before stated, palaeontological nomenclature is simple and well established. No one, I think, will dispute that generic and specific names of fossils must always be in italics, and it is a useful rule, consistent with most biological practice, that generic names must always begin with a capital letter, and specific names never, even when they are derived from personal names, like *murchisoni*. If this rule is not adhered to a tabular list of fossils looks deplorable. Names of families, e.g. Retiolitidae, and all higher zoological divisions should be in ordinary roman type. Then again, biological and palaeontological authors have a mysterious system of their own, which the present writer has never understood, as to when the names of authorities for fossils are enclosed in parentheses and when they are not. By the ordinary rule, a thing like (Sow.) appears to indicate that the author referred to did not call the fossil by the name quoted, but something else: perhaps, however, this is sheer ignorance, like Dr. Johnson. It might be suggested, with much diffidence, that when the name of a particular fossil comes over and over again in the same discussion it can hardly be necessary to quote the authority (with or without parentheses) every time. It is not likely that the author, without saying so, should have branched off to the consideration of another fossil of the same generic and specific names.

The next broad question that arises is as to the capitalization of the names of rocks and minerals. This is a debatable subject, and must of necessity be more or less a matter of choice and opinion. After much careful consideration, and a certain amount of variation in practice from time to time, the present writer has come to the conclusion that such words should *never* be written with capitals (except, of course, at the beginning of a sentence). This seems to conform best to the English convention, which is the exact opposite of the German system: at any rate the rule is simple and universally applicable, leaving no room for doubtful cases. It is admittedly difficult to persuade many authors to write, for example, kimberlite with a small *k*, but it must be done.

The problem of the capitalization of stratigraphical names is much more difficult, and has led to endless trouble and confusion. The special difficulty can, perhaps, best be illustrated by means of actual cases. Take, for example, " dolomitic conglomerate " or " Dolomitic Conglomerate ". The first of these is a petrographical term of perfectly general application, while the second is the stratigraphical name of a particular sedimentary deposit found in Somersetshire. Again, " greensand " is a general lithological term, while several varieties of " Greensand " indicate certain subdivisions of the Cretaceous of Britain. The words " Carboniferous Limestone " written with capitals are universally accepted as indicating the British marine facies of the lower division of the Carboniferous system, whereas " a Carboniferous limestone " may indicate a calcareous stratum found in any part of the Carboniferous system. Innumerable similar instances will present themselves to the reader and the point need not be laboured further.

From such instances, however, it does seem to be possible to draw up a general rule, as follows:—

When the group of words form the name of a particular stratigraphical division, *all* of them should be written with capital letters.

Conversely, of course, when the reference is to a lithological type found in a particular stratigraphical system, the lithological (petrographical) name should not have a capital, as set forth in the section on petrographical nomenclature.

This is, perhaps, the most troublesome of all technicalities in geological typography, since it is so difficult to drive the point home to editors of general scientific publications of a non-geological nature.

Now the Editor is well aware that in this respect the practice of the GEOLOGICAL MAGAZINE in the past has been far from consistent. No doubt this inconsistency is largely due to slackness on his own part, but this is no reason against improvement in the future, and an earnest appeal is hereby made to authors to endeavour to carry out in their manuscripts the principle set forth above.

It may be well to mention here a point of quite general application; namely, that in verbatim quotations from earlier authors, the passage should be copied exactly as it stands, typographical errors and all. It is quite inadmissible to touch up such passages in conformity with modern usage; such a procedure might also conceivably be unfair to the original author.

I venture to think that one of the most unfortunate terms ever introduced into geology is *oolite*. The original idea was no doubt excellent: certain Jurassic limestones do strongly suggest petrified fish-roes: hence *roe-stone* or *egg-stone*. But the Greek word for a stone is *lithos*, not *litos*. Therefore, it may be asked, why, for the last hundred years and more, have geologists persistently dropped their aitches? The word *Oolite* as a stratigraphical term has held sway for a very long time; it may happily now be regarded as obsolete, but its evil influence has persisted in other directions. In the petrographical description of rocks showing this type of microspherulitic structure, each unit should be described as an *oolith*, a stone egg. This is a point of considerable importance, since it figures largely in the discussion of the origin of "oolitic ironstones" (which, of course, ought to be "oolithic ironstones"), a subject of high technical and economic interest.

By analogy this paragraph suggests another, with reference to certain words widely used to describe the forms of igneous intrusions. It is unfortunately true that the original form of the earliest and commonest of these is *laccolite*, but with all due deference to many eminent authors, this is quite indefensible, for precisely the same reasons as apply to oolite. The derivation is from *lakkos* and *lithos*, a stone cistern, and the only possible spelling for such words is laccolith, bysmalith, and so on. It does not seem to matter much whether we write batholith or bathylith. According to *Liddell and Scott*, the noun is *bathos*, the adjective *bathys*. As the real meaning is "a deep rock" perhaps bathylith is to be preferred. The word dyke or dike means such a lot of different things in different places that its spelling may well be left to individual choice.

We now come to the most potent source of variation and trouble in manuscripts and type, namely, the method of quoting authorities in bibliographies and footnotes. Nearly every journal has a different standard form, for most of which something may be said, and it is with much diffidence that I venture to approach this thorny subject. But as a matter of fact for years past I have devoted a great deal of thought and attention to the working out of what seems to me the best method, and I venture to think that my mature views are at any rate worthy of serious consideration, even if the system here suggested does not agree with the practice of certain well-known geological journals. The main points kept in view in this discussion are clearness, simplicity, and ease of reference. It is necessary to consider what convention will make it most easy to turn up a reference in any properly arranged geological library. Such references may in the vast majority of instances be relegated to two categories: (*a*) books, and (*b*) papers in periodical journals.

In the case of a book the matter is fairly plain sailing. The data required are: the name of the author or authors, the title of the book, the edition, if more than one, the place and date of publication, and the page, plate, figure, map, etc., specifically referred to. Here is an example—

Woodward, H. B., *The Geology of England and Wales*, 2nd edition, London, 1887, 270.

This is a reference to the general account of the Middle Lias in this well-known book. In many instances the name of the publishers is useful, though not really essential. It is to be noted that in all cases the title of the book, as it appears on the title-page, should be in italics; all the rest in roman type. The question of volume-numbers in books and publications of more than one volume will be discussed later. The initials of the author should always be given (after the surname as above), since in geology and doubtless in other sciences there are often many writers of the same surname. The example here quoted is a specially good instance of this fact. Frequently it is possible to omit a large part of the title of the book without any deleterious effects: in old books especially the real title expressing the scope of the work is often followed by a long and often unnecessary sub-title in smaller type: e.g. "Darwin, C., *The Origin of Species, by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life.*" In this case only the author's name and the next four words are necessary, and similarly in other instances, which may be left to the discretion of the writers who quote them.

Before passing to the consideration of references to periodicals in the strict sense, it may be well to discuss the matter of volumes of a specialized nature (monographs, if the term is preferred) which form part of a series. Such are the *Memoirs of the Geological Survey of Great Britain.* It is possible to treat these in many different ways, but it seems to the present writer that the simplest plan is to deal with them and with similar publications as if they really are constituents of a periodical series, and to write the references in the same general form, which will be explained shortly. Thus, by anticipation, the reference to one of the best known of these will come out as follows:—

Harker, A., "The Tertiary Igneous Rocks of Skye," *Mem. Geol. Survey*, 1903.

When these *Memoirs*, as is so often the case, are due to a number of authors, discretion should be used as to whether all the authors are named, or the reference is given as " A. B. and others ".

A special case arises in connection with the dates of publication of the annual *Summary of Progress of the Geological Survey.* It is obvious that the summary of progress for any particular year cannot be published until the next year at the earliest, and it is sheer pedantry to insist on quotation in such a form as " *Summ. Progr. Geol. Surv. for 1928* (1929) ", and so on. The second date merely leads to possibility of confusion and is entirely useless. What geological science wants to know is, what happened during 1928 : the exact time at which this information was given to the world cannot matter to anybody, so long as it is available.

By far the most important subdivision of this subject is that of reference to individual papers in recognized periodicals, and in this there is the widest scope for variation, as may be easily seen by the most casual inspection of current geological and other literature. The whole thing is in a state of chaos. Now it is obvious that in a matter of this sort there can be no absolute standard form : it must be regulated by convention and by convenience, and any recommendation made must be more or less dogmatic. The practical point concerned is really this : when a reference is to be given, what form will most readily enable the reader to find the volume he wants on the shelves of a properly arranged library ?

Now the factors here concerned are the following : name of author ; title of paper ; name of journal ; number of volume ; year of publication ; page ; and in special cases, number of plate, figure, map, diagram, and so on.

The first of these presents little difficulty : there can as a rule be no doubt as to the name and initials of the author or authors, and initials should follow the name, as before described. In the majority of cases it is highly desirable to give the title of the paper in full, or if the title is very long, at any rate the significant parts of it. Now here it is necessary to introduce a bit of dogmatism, resulting from long experience : the title should be in roman type, between inverted commas (" quotes," in the horrible jargon of the typewriting office). It is hardly necessary to say that such matters as capital letters should be copied exactly from the original. If, however, as often happens, the whole title is in capitals, the author must use his own discretion : some help can often be obtained from an index or table of contents in the volume. As a matter of fact the correct or, at any rate, the standardized use of capital letters in titles of books and papers is one of the most difficult matters that an editor has to decide, and it must often be very arbitrary. In German and French the matter is much simpler : all German nouns must have capitals, and in both languages adjectives never.

Next comes the title of the periodical. Again, dogmatically, this should be in italics, and in nearly all cases it can be abbreviated. For this purpose I have no hesitation whatever in recommending as the standard form of abbreviations the lists prefixed to the annual *Geological Literature added to the Geological Society's Library.* These lists are obviously the result of wide linguistic knowledge and careful scholarship, and could hardly be improved upon. Personally, I greatly dislike the extreme forms of abbreviation so commonly used, such as *Q.J.G.S.*, *A.J.S.*, *C.R.*, *Z.d.g.G.*, and so on: they look slipshod, and the extra cost of printing standard forms is negligible.

The next matter on the list, the numbering of the volume, is one that, somewhat surprisingly at first sight, gives rise to much trouble. This is due to the unfortunate fact that many periodicals divide themselves up into series, decades, and so on. This is most objectionable, and I regret to have to record that for more than fifty years of its existence this Magazine was a notorious offender in this respect. However, when the control came into my hands this was very quickly stopped. The first volume that has a proper number was the fifty-sixth, for 1919, and it is hereby requested that in all citations of earlier volumes of the GEOLOGICAL MAGAZINE, references to decades be omitted and nothing but the year given. I would like here to pay a tribute of admiration to the *Philosophical Transactions*, the *Comptes Rendus*, and others which have had the courage to run into hundreds of volumes without any break, though the first-named has rather spoiled the effect by having two categories running parallel, as it were, " A " for the physical and " B " for the biological sciences. The method of writing the number of a series, when such must be given, will be discussed shortly.[1]

It is necessary at this point to deal with a subject of much importance: that is the question whether it is really necessary every time to write out series, volume, page, and so on, or even any abbreviation of them. My attention was called to this matter by a casual remark of a valued contributor that in a certain paper all this took up a lot of space, as it seemed unnecessarily, and I have since thought it all out very carefully. The conclusion arrived at is that, with a properly standardized typographical scheme, all of these can with advantage be omitted, as is indeed often done, especially in American publications. Furthermore, I have consulted the printers of this Magazine on one or two points. There is much variation in the practice of various journals as to the order of the items, volume-number, date, and page. The order finally adopted here is that just given, and, moreover, I personally have a rooted objection to the common habit of enclosing the date in parentheses. They seem to interrupt the sense and relegate the date to a position of minor importance, whereas in running the eye along a series of bound volumes on a library shelf the year is in reality the thing looked for. Possibly others as well as myself may occasionally feel a little uncertain as to whether vol. xlvi, for example, comes before or after vol. lxiv. The year should always be stamped clearly on the backs of bound volumes. There is no point in quoting the numbers of the unbound parts of a periodical if the number of the volume is given correctly.

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[1] When this article was already in type the Editor came across an amusing instance of the disastrous effect of the existence of series, decades, etc. A reference in an Indian publication to the volume of the Geological Magazine for 1911 got transmogrified into "December 5, VIII, page so and so", which is not very illuminating.

The next point is the page-number. The essential thing here is the page at which the paper begins, and I see little advantage in quoting the last page as well. It by no means follows that a long paper is more important than a short one: in fact the reverse is often the case. Besides, there is sometimes a difficulty in deciding where a paper really stops, when there are appendices and a printed discussion. The case of those periodicals which have two or more paginations running in the same volume is too complicated to be discussed here. It can only be deplored.

The following are examples of references given in the form here suggested, chosen at random from current geological literature:—

Lamplugh, G. W., "On the Speeton Series in Yorkshire and Lincolnshire," *Quart. Journ. Geol. Soc.*, lii, 1896, 179.

Woolacott, D., "On the 60-foot Raised Beach at Easington, Co. Durham," *Geol. Mag.*, lix, 1922, 64.

Hall, T. C. F., "The Petrology of the St. Austell Granite," *Proc. Geol. Assoc.*, xxv, 1914, 180.

To sum up, the types suggested for the numerical parts of references are: Series, heavy type; volume, small roman numerals; date and page, arabic figures; plate, small roman; figure, arabic: thus (an imaginary example):—

5, vii, 1922, 147, and pl. iv, fig. 3.

But in references to the Magazine itself the style should be:—

Geol. Mag., LXX, 1933, 230, and Pl. VII, Fig. 2.

With regard to the preparation of manuscripts, after consultation with our printers, the following rules may be laid down:—

Single underlining means *italics*.
Double underlining means small capitals.
Wavy line means **Clarendon type.**
Triple underlining means LARGE CAPS.

Finally, I desire to thank Messrs. Stephen Austin & Sons, Ltd., for help most kindly given in the preparation of this article.

The Steinheim Basin and the Pretoria Salt Pan. Volcanic or Meteoric Origin?

By Herbert P. T. Rohleder, Ph.D. (London and Munich), D.I.C., F.G.S., Tanga, Tanganyika Territory.

WHEN looking through the bibliography referring to the cryptovolcanic phenomena of Swabia we find that it has been quite customary to explain the origin of the " Steinheim Basin " by the well-known facts which have been ascertained in the Ries of Nördlingen. When doing so one important fact has been deliberately ignored, viz. that only the Steinheim basin is actually cryptovolcanic, whereas the Ries of Nördlingen exhibits undeniable proofs of extensive volcanic activity. There are rhyolitic tuffs, weathered-out lava cords and lava slabs, partly absorbed crystalline rocks, *et alia*. In the case of the Ries of Nördlingen the explanation of the vast caldera and the various geologic abnormalities must obviously be sought in volcanic activity.

Again, in the case of the Salt Pan, north of Pretoria, attempts have been made to solve the problem of this isolated crater by comparison with other better known specimens of " sunken calderas ".

But in both cases there is one great difficulty which cannot be explained away. If these craters are due to phreatic volcanic explosions, how is it that not a single token of this activity has been left ? There are no lava, no bombs or lava cords or volcanic breccia (Bunter Riesschutt) as in the Ries ; there are no signs of contact metamorphism as would be expected in the case of volcanic origin. There is merely a basin-like depression which resembles a crater, nothing but a geomorphological feature, around which the volcanic origin has been built up.

Thus it is justifiable to apply other theories of origin. Within recent years meteor craters have been found and studied in many parts of the world. The best known example is the meteor crater near Phoenix in Arizona, an isolated round hole approximately 500 feet deep, and with a diameter of 4,000 feet. This, and also all other meteor craters so far known, have recently been described by Dr. L. J. Spencer, and in spite of the fact that Dr. Spencer gives a warning in this paper not to regard all crater-like depressions as " meteor craters ", the writer is convinced that the evidence for the volcanic origin of the Steinheim Basin and Pretoria Salt Pan is of a decidedly doubtful nature.

I. THE BASIN OF STEINHEIM IN SWABIA (SOUTH GERMANY).

The basin of Steinheim, approximately 4 miles (7 kilometres) west of Heidenheim in Württemberg, is a circular depression within the flat Swabian Jurassic limestone country, the so-called " Alb ". The diameter of this crater-like hollow is approximately 8,000 feet

(2½ kilometres), and the depth nearly 250 feet (80 m.). There is no rim around this depression, but it has two outlets, one in the south and one in the south-east.

In the centre of the crater there is one isolated hill, the Klosterberg, 120 feet high. Thus Klähn divides the Steinheim basin into three parts; the centre or core (Klosterberg), the peripheral depression, and the marginal zone.

Contrary to the Swabian Alb, which in the vicinity of the Steinheim basin shows no geological abnormalities whatever, the rocks consisting of Upper Jurassic limestone, the "Weisser Jura" or Malm, the Steinheim basin itself exhibits the most noteworthy phenomena. The geomorphological features are outstanding, the basin closely resembling a "sunken caldera"; within the latter is the Klosterberg cone, giving the impression of a small volcano of the Vesuvius-Somma type. The bottom and also the edge of the basin consist of Upper White Jurassic limestone; only recently has a patch of older material, Brown Jura (Dogger) and Black Jura (Lias), been located by Bentz.

The Klosterberg itself has been proved to consist of a mixture of different rocks—Upper, Middle, and Lower Jurassic. Although within certain parts of this mass the rocks appear to be in situ, nearly all authors agree that this Klosterberg raft is an "alien", a mass of older rock resting on younger formations and originating from a much lower horizon. The most startling incident, however, is the fact that not one piece of true volcanic material has ever been found within or around the Steinheim basin. But up to now all authors have tried to explain the geological features of Steinheim by volcanic activity, and owing to the obscure nature of the volcanic activity Branca and Fraas gave this type of vulcanicity a special name: "Kryptovolcanic."

A strange phenomenon within the shattered Upper Jurassic limestone is afforded by certain striated surfaces, which were first described by Branca and Fraas, as resembling some problematic algae like *Cancellofycus*, *Taonurus*, etc. Such marks have never been found elsewhere, and are generally regarded as effects of volcanic explosions. W. Kranz calls such fragments "volcanic bombs", in spite of the fact that they consist of pure white limestone.

After the basin had been formed, the latter was partly filled with water, as is shown by considerable occurrences of highly fossiliferous, calcareous fresh-water deposits of Upper Miocene age, the well-known Steinheim fauna. There are also occurrences of tufa with aragonite, the latter generally being interpreted as hydrothermal deposits during the period of expiring volcanic activity.

There are various theories concerning the origin of this basin.

E. Fraas was the first to realize that the "Dogger" of the Klosterberg rests on Lower Malm, but he failed to give a satisfactory explanation.

Branca and Fraas believed that a laccolith domed up the centre of the Steinheim basin, whereas the peripheral parts sank. Thus the Klosterberg strata were supposed to be oriented like an anticline and this tectonic movement to have taken place before the formation of the fresh-water deposits.

Later, Branca revised his theory by stating that in addition to the doming up caused by the laccolith certain " contact explosions " took place, which account for the abnormal position of the " Dogger " within the basin. However, this reasoning is anything but clear.

W. Kranz tried to prove that the Ries of Nördlingen and the Steinheim basin were caused by great explosions near the surface. Following a suggestion of Albrecht Penck, Kranz made experiments with a model of the Ries and in fact succeeded in obtaining very similar results. According to Kranz, two explosions took place during the later Upper Miocene. By volcanic gas and steam certain parts of the former surface were removed and thrust over other parts, large masses of rock were brecciated and powdered, and also the strange striated fractures, the so-called "Drucksuturen", were formed which were first described by Branca and Fraas. The Klosterberg, according to Kranz, is formed of irregular masses of material which were cast out by the explosion and fell back into the interior of the crater. Kranz denies emphatically that the Steinheim basin fits into the tectonic structure of the neighbouring Alb.

According to Klähn, the historical events during the Upper Miocene age should be subdivided thus :—

(1) Doming up of the whole basin, caused by the laccolith; formation of Upper Miocene fresh-water deposits; followed by breaking up of the country through tectonic processes (" Graben ").

(2) Second tectonic phase: The rift valleys are cut deeper, especially around the Klosterberg; hydrothermal action; further formation of fresh-water deposits.

(3) After the sedimentation process had ceased, further tectonic movements took place.

A. Bentz, 1923, finally pointed out that the difference of opinion between Kranz and Branca is not fundamental, as in later years Branca fully admitted the possibilities of an explosion near the surface. According to Bentz, the geomorphological conditions of the Steinheim basin in Miocene times might have been such that the explosion actually did not remove such vast masses of rock as was generally believed.

So far the different authors agree in one point, i.e. that the origin of whatever caused the Steinheim basin should be sought below. With the exception of Klähn, they also state that the key to the correct interpretation is the famous Ries of Nördlingen. By submitting the new meteoric theory the author ventures to break with both traditions.

There is no reason why one should try to explain the Steinheim basin by the Ries of Nördlingen. The Ries of Nördlingen is over ten times as large as the basin of Steinheim. The Steinheim basin is nearly 20 miles south-west of the Ries, and therefore could never be regarded as a direct offshoot of this caldera.

In the Ries there are many occurrences of real explosive material, rhyolitic tuffs with inclusions of true magma, lava cords and lava slabs. At Steinheim there is nothing of the sort. In the Ries the pre-Cambrian crystalline rocks have been partly absorbed, and now form a hybrid rock. No crystalline rock, whether altered or unaltered, has ever been found in or near the Steinheim basin. In the Ries basin, there are many occurrences of a volcanic breccia (called " Bunte Brekzie " or " Bunter Riesschutt "), consisting of many different rock types found within the area, sediments, crystalline and metamorphic rocks; there is none at all at Steinheim, although it is obvious that the vent would have pierced approximately the same geological horizons as at the Ries.

In a concentric ring following the outlines of the Ries caldera, a district known as the " Vorries ", the influence of the great catastrophe which led to the formation of the Ries can be studied, decreasing in intensity the further away from the centre of volcanic activity.

As soon as the margin of the Steinheim basin is left, there is nothing at all to point to any anomalies. The " Alb ", even in the immediate vicinity, is absolutely undisturbed.

Even Branca and Fraas point out that " overthrusts " are to be found within the Ries basin, but not at Steinheim; that floating slabs of Dogger occur *in the neighbourhood* of the Ries, but only *within* the basin of Steinheim; that the strange striated surfaces within the Upper Jurassic limestone (" Drucksuturen ") of the Steinheim basin have so far never been traced within or around the Ries. The same authors state that the type and degree of fracturing of the sediments (" Vergriesung ") within the two basins are different, and it is also interesting to recollect that within the Swabian district of 127 volcanic vents, described by Branca, i.e. the district of Urach-Kirchheim, there is not one single occurrence of " Vergriesung "; and finally, Haussmann comments on his geophysical investigations: " At the Ries there are masses of basic rocks not more than 2 kilometres below the surface, whereas the maximum of disturbing influences (near the Steinheim basin) appears to be between Heidenheim and Giengen; whether the Steinheim basin takes part . . . cannot be decided." Kranz summarizes this as follows: " The lines of disturbing influences simply ignore the existence of our basin."

Thus one is justified in saying: *The Ries of Nördlingen is a highly differentiated complex, the volcanic origin of which is obvious although very difficult to explain; whereas there is nothing whatever to prove the volcanic origin of the Steinheim basin, and the fact that the immediate* *neighbourhood was in no way involved seems to point out that whatever caused the basin was an absolutely local event.*

And such a "local event" would surely be the impact of a meteorite. The size and depth of the crater are not much more than those of the Arizona crater and decidedly less than the occurrence in West Africa (Ashanti).

The shattered rocks ("Griesbrekzie") at the bottom of the crater would be comparable with the shattered rocks known from all meteorite craters. The irregular masses of older rocks in the centre and also the patch of Dogger at the edge of the basin, found recently by Bentz, are probably remnants of masses which were tossed up by the impact and fell back to the ground. Remnants which fell over the edge of the basin have probably been removed by erosion and denudation, as has been pointed out by Kranz and others.

Tufa with occurrences of aragonite show that the temperature of the water within the basin must at times have exceeded 30° C. So far all German geologists have regarded this fact as strong evidence for the volcanic origin. Needless to say aragonite does occur in many sedimentary rocks without any connection whatever with volcanic activity. But if this occurrence within the Steinheim basin has so far been regarded as so important, one is surely equally justified in assuming that the source of heat which gave rise to this mineral was not volcanic activity but the meteorite cooling in depth. The amount of aragonite is scanty, and we know that the cooling of igneous bodies can take a considerable time.

The negative results of Haussmann's geophysical investigations show that there cannot be a reservoir of basic magma under the Steinheim basin, as has been supposed by German geologists. The mysterious absence of true volcanic rocks, the greatest difficulty in all former theories, would thus be easily explained, but instead, one might expect remnants of the meteorite on the surface, which so far have been found at practically all meteorite craters.

But firstly we have to consider that the impact took place in Upper Miocene times; although one would expect at an early stage a certain amount of meteorite debris in the vicinity of the crater and at the final stage, that the body itself should be exposed, it is obvious that there would be a long intermediate stage, during which no traces of either could be noticeable. The bottom of the crater has in addition been covered up by later sediments. Finally, Dr. Whipple has rightly pointed out that in addition to the hitherto known iron meteorites, there are numerous stone meteorites to be expected; as Whipple says: "It is easier to blow a stone to smithereens than iron," and this fact alone could account for the complete absence of meteorite remnants.

Therefore, it is probable that the Steinheim basin resembles one example of Spencer's group of single meteorite craters without associated meteoritic material.

It was only after I had finished this manuscript that Professor Keilhack showed me in Berlin a new publication by J. Kaljuvee of Tallinn (Reval). This Esthonian teacher, as clever as bold, tries to explain many geological features of the earth's crust through impacts of large meteoric bodies. As such he not only regards the Steinheim basin, but also the Ries of Nördlingen, in spite of its vast size—the diameter of the crater being 15 to 20 miles. He goes even so far as to interpret the volcanic rocks of the Ries and also those of the 127 embryonic volcanoes of Swabia as effects of this vast Ries meteorite impact, the volcanic material having been squeezed to the surface in places of minor resistance.

Although I personally regard this latter hypothesis as extremely bold, it seems only fair to state the fact that Kaljuvee's ideas, as far as the origin of the Steinheim basin is concerned, are identical with mine.

II. THE SALT PAN NORTH OF PRETORIA (TRANSVAAL).

The "Salt Pan" about 25 miles north-north-west of Pretoria is a flat-bottomed amphitheatrical depression, surrounded by an almost perfectly ring-shaped ridge which scarcely rises from the tree-clad, gently undulating Bushveld of the Transvaal. The floor of this depression is generally covered in its outer portion by a white saline incrustation and towards the centre by a dark pool of soda salt brine.

The diameter of the great hollow measured from rim to rim along a north-south line is 3,460 feet and along an east-west line 3,330 feet, the circumference being 11,100 feet. The bounding ridge has an average elevation of some 100 feet above the level of the neighbouring country and of about 300 feet above the floor. The highest part of the ridge (200 feet above the country around and 404 feet above the floor) is situated on the north-west side.

Geologically the depression is situated in the heart of the igneous complex of the Bushveld, the country being occupied by red Bushveld granite, worn down to a fairly level surface, on which rest isolated outliers of grit belonging to the Coal Measure series of the Karroo system.

The rim of the Salt Pan is composed of coarse uncemented breccia made up almost exclusively of large and small angular blocks of red, brownish, or pink hornblende-biotite-granite resting on grits. Large boulders of granite at parts of the top of the ridge, which were ejected from the centre, have resisted denudation. The detritus is clearly in exactly the same position in which it fell after being forcibly expelled from the centre of the depression. Some of the fragments are grooved and striated, but the granite shows no sign of chemical alteration. Beneath the layer of brecciated granite, which shows a maximum thickness of 125 feet, the rudely stratified grits, which occur only on the southern side of the rim, consist of subangular and rounded grains of quartz set loosely in a scanty matrix of white clayey matter. The thickness of the grits is approximately 30 feet.

The granite underlying the grit is, in contrast with that of the ejected blocks, much decomposed, being of greyish-pink or grey colour, owing to the kaolinization of the felspars. Due to shattering and compressional shear, the granite presents all the phenomena characteristic of a massive homogeneous rock that has been subjected to compressive stress in the upper part of the zone of fracture.

Among the detritus there is not one specimen of young volcanic rock. In addition to the brecciated granite, many fragments of which are traversed by narrow fissures filled with a breccia composed of angular fragments of quartz and felspar, there are grits, some of which belong to the Coal Measure series of the Karroo system, showing that some of the ejectamenta must have been derived from this horizon.

In addition to these there are a number of igneous rocks probably related to the granite, and also a few types of sedimentary rocks; among the latter a dolomitic breccia is of importance as, according to the late P. A. Wagner, it is the only rock that bears any resemblance to a volcanic rock. Although a full description of this rock is given by this author, there is no reason whatever to suggest volcanic origin, beyond the fact that the rock has been thoroughly broken up by some unknown phenomenon.

Three possible theories are given by Wagner to account for the origin of this breccia :—

(*a*) That it is of igneous origin and of the nature of an injection breccia formed by the incorporation in a magmatic dolomite-calcite-biotite-magnetite-ilmenite-pyrite-apatite rock—of fragments of chlorite-schist and chloritic quartzite. Rocks of this type have been described as sovites and rauhaugites by Brögger and Hogböm from Norway. But Wagner admits that in this case the entirely unaltered condition of the quartz scattered through the carbonates would be difficult to explain.

(*b*) That it is of igneous origin, but intensely altered, having been formed by the almost complete replacement by carbonates of a chlorite-rich porphyritic rock, such, for instance, as a melilite rock.

Again, the author sees difficulties in explaining how a rock could be so uniformly and completely replaced, particularly in view of the relatively unaltered condition of the chlorite and quartz and the rather unusual composition of the carbonate matter.

(*c*) That it is of the nature of a dolomite-cemented, volcanic breccia deposited on the surface of a red granite. Here the author cannot account for the presence of a certain chlorite-schist, obviously an ancient metamorphic rock, of which the breccia contains such an abundance.

Although Wagner finds contradictions in all three theories, he is inclined to believe that the dolomitic breccia is of the nature of a xenolithic carbonatite (rauhaugite) intrusive in the red granite.

It is, however, equally justifiable to dismiss all the three theories of volcanic origin and state the simple fact that the rock in question is a purely mechanical breccia containing an unknown chlorite-schist not outcropping in the district, the whole resembling to a great extent the " Gries " phenomenon, already discussed in the Steinheim basin.

Two points in regard to the distribution of the non-granitic ejected blocks are described and interpreted by Wagner as follows :—

The first that such blocks rarely occur singly but usually in the neighbourhood of a number of boulders of the same material, many of which are clearly attributable to the breaking up by weathering of a single large block. In other instances Wagner regards this distribution as the natural consequence of rock from the same horizon having been thrown up in the same direction and to the same height.

In the case of a phreatic explosion, as hitherto suggested by Wagner, one would not expect a small local gathering of one special rock type, but a thorough mixture of all rocks concerned, tossed into the air by the eruption and distributed within and around the crater according to the laws of gravity, not to their petrological nature. The impact of a meteorite, however, would resemble in many ways that of a shell, and in such a case certain directions of flight of the same material would be more plausible.

The second point is that such blocks are undoubtedly more abundant in the upper part of the cone and in the fallen material derived from it than in the lower part. Wagner explains this by the fact that rocks thrown into the air would fall in the reverse order in which they were pierced. Another equally justifiable conclusion is that, if ejectamenta of greater depth are rarely to be found, this might be due to a body burying itself in the earth from above more likely than to a phreatic explosion, which in greater depths was even more active than close to the surface.

Referring to an occurrence in the Auvergne not unlike the Salt Pan, Wagner discusses the possibility of a layer of volcanic debris originally covering the Salt Pan ; but he rightly misses any traces of the latter at the surface in the different boreholes which were put down on the inner slopes of the so-called caldera, and in particular in two boreholes (9 and 10), where layers of coarse detritus were struck in depth. If Wagner concludes that on account of the absence of any traces " the question of whether or not a layer of scoria originally covered the cone must thus remain open ", it seems more justifiable to state that there is not the slightest reason to assume that such a layer of scoria ever existed.

The breccia composed of slipped and fallen blocks, with disintegration and decomposition of the rocks, mainly the granite, is, according to Wagner, " probably petrographically indistinguishable from the explosion debris building the encircling ring, being composed of ejected material which after being thrown out by the vent fell back into it." In other words: As no fragmentary material can be separated in any way from the detrital material, the origin of which is merely due to mechanical breaking up, there is again no reason whatever to assume volcanic origin.

Wagner assumes that neither the original depth of the crater nor the original height of the ring around it exceeded the dimensions considerably, as compared with present conditions; such inconspicuous cones are characteristic of explosion craters the world over, the reason probably being that the ejectamenta are scattered over a wide area as a result of the violent paroxysmal nature of the eruptions to which the craters owe their origin. But if this were so, the fact that not a single trace of such ejectamenta can be found seems more incredible; besides, boreholes have so far been put down through 230 feet of bedded muds and clays with intercalated layers of granite detritus, without striking the explosive material, which is supposed to occupy the upper portion of the vent.

Wagner summarizes the mechanism of the Salt Pan eruption as follows:—

The Salt Pan is a "sunken caldera", the great hollow having been formed by the engulfment within a ring-shaped fault of the central part of a phreatic volcano formed by a violent gaseous eruption. As to the nature of the gas . . . the complete absence of all signs of alteration or metamorphism suggests that it was steam, possibly in connection with carbon dioxide formed by the dissociation of magmatic carbonates. No lava rose in the pipe, there were apparently several explosions, the final ones occurring at a considerably lower level than the earlier ones, this being in the upper part of the norite zone of the Bushveld complex, or approximately 10,000 feet below the Salt Pan.

The Salt Pan is decidedly of post-Karroo age, and judging from the well-preserved condition it is young, certainly post-Cretaceous and probably of late Quaternary age. Wagner continues:—

"Perhaps the most remarkable feature is the complete isolation from other centres of igneous activity, this being the more surprising inasmuch as phreatic vents are generally clustered together, as witness the kimberlite pipes, the volcanoes in the neighbourhood of Sutherland in the Cape Province, and the explosive craters of the Bavarian Alb." (Swabian Alb is meant.)

There is no obvious reason for the Salt Pan pipe being situated where it is. It is not visibly connected with any line of fissure or faulting, having apparently drilled its way to the surface independently of any plane of structural weakness in the earth's crust . . . there is just a possibility that the Salt Pan may be the southernmost of the East African volcanoes.

This suggestion, however, seems rather far-fetched. For how could one expect this one, isolated, minute occurrence of volcanic activity in the heart of the South African Union to be in direct connection with the East African "Graben"-vulcanism when there is not the slightest sign of recent volcanic activity in Southern or Northern Rhodesia and the southern parts of Tanganyika Territory.

Thus the outstanding features of the Salt Pan are the circular depression and shattered rocks on the bottom of the latter and in the vicinity. Nothing whatever suggests volcanic origin, but the parallelism with a meteor crater is obvious.

The possible meteoric origin of this crater was—unknown to the author—recently suggested by Dr. L. J. Spencer.

Finally, the author wishes to acknowledge with gratitude helpful criticism and advice from Dr. L. J. Spencer, F.R.S., Geheimrat Keilhack of Berlin, and Mr. Ernest F. Fox, Cambridge, Mass.

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A Search for the Mechanism of Earth Movements.

By R. G. Lewis, F.R.G.S.

THE first two reports of the Terraces Commission of the Union Géographique Internationale[1] contained sufficient material to enable an examination to be made of the spacing of raised beaches at various places, in the endeavour to discover some mathematical relationship between their heights above sea-level. As in some cases river terraces reflect base-level movements very faithfully, but for various reasons this is not always so, attention was at first confined to sites open to the oceans.

The method employed was twofold: firstly to divide the height above sea-level of a beach into the height of the beach next above it—and repeat this process for succeeding beaches—to see if any constant proportion could be found; secondly, to plot a curve for each site having as ordinates the heights of the beaches (including in this term erosion platforms and other features which imply alteration of base-level), the abscissæ being, in the absence of other indications, equally spaced. Figs 1 and 2 show two curves plotted in this way, no constant proportion between the heights of the beaches being evident. It will be observed that the curves show similarity and that they are of characteristic shape.

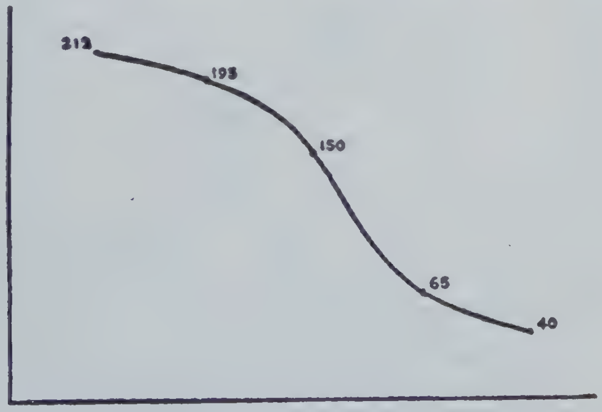

Fig. 1.—River Ibaizábal (Bilbao), Spain. *U.G.I.*, ii, 8.

In cases where, in the reports, heights are given between limits, e.g. 700–600 metres, or, to quote a special case, that of the Nive terraces,[2] 15–17, 26–34, 40–57, 80–95, 142 metres, both the higher and lower limits were plotted and joined by a line. At or near the lower limits of the Nive measurements the series 15, 26·2, 45·8, 80·1, 140 gives a constant proportion of 1·75, and Fig. 3 shows the shape of this curve. Although the range of movement is of the same order as that at Bilbao (to use this name for the Ibaizábal site), the characteristic behaviour is different.

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[1] Quoted hereafter as *U.G.I.*, volume and section.

[2] *U.G.I.*, i, 9.

In cases where a constant proportion could be discovered between the heights of adjacent beaches of a series but gaps occurred, spaces were left on the supposition that all traces of base-level at such a point may have disappeared or remained unrecorded. Fig. 4 was prepared from Krige's observations in South Africa [1] and the curve shows gaps at A, C, and D in a series which is otherwise complete from 12 to 4,000 feet. This remarkable curve has a characteristic similar to that in Fig. 3, the implication of the important difference in behaviour between these two and the first two curves being discussed below.

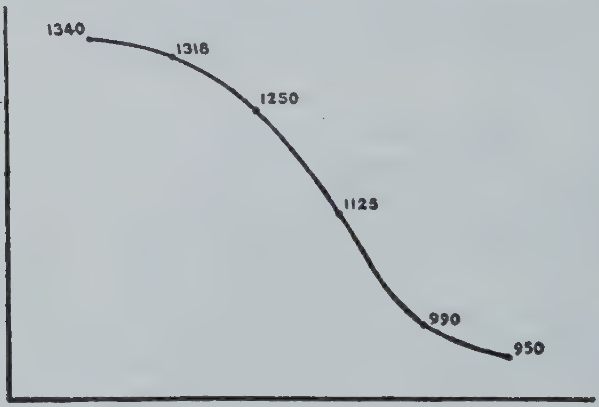

Fig. 2.—Mt. Voïlias (G. of Corinth), Greece. *U.G.I.*, i, 4.

In the search for a reason for the rhythmic disposition of beaches it must not be forgotten that while most of the evidence is of features above present sea-level, submarine features, though naturally not easily observed, are occasionally noticed ; and Krige [2] mentions the Agulhas Bank, a submerged shelf between Cape Town and Port Elizabeth. The soundings are given as from 40 to 60 fathoms, and it is remarkable that 220 feet, which is about the lower figure, and 373 feet, which is about the higher, are in the proportion of 1·7, and would fit the curve in Fig. 4 inverted at points respectively half-way between the 450 ft. beach and B and between B and the 150 ft. beach. Further, he remarks on the occurrence of Recent or

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[1] Summary, *U.G.I.*, i, 19.

[2] *Annals Univ. Stellenbosch,* 5th May, 1927.

Pleistocene shelly sands at -122 feet in the Buffalo River, and similar deposits at -100 feet in the Keuerbooms River. Now 130 stands in the proportion of 1·7 to 220, and would therefore fit the same curve inverted, half-way between the 150 ft. beach and C.

There are also, in the Dancala Depression, which is in Eritrea near the Red Sea and the border of Abyssinia, terraces below present sea-level. Stefanini, in his communication to the Terraces Commission,[1] relies chiefly on the work of Vinassa and Marinelli. Vinassa recognized terraces in the central and northern parts of the basin at 180, 120, and 30 metres above sea-level, of marine origin, which seem to be of the nature of platforms. If the watershed at Beheita at 80 metres is included and the lower of the two level summits at 200 metres, this gives a set of five above sea-level. Then there is a beach circling the contour at sea-level " rich in fossils which are . . . identical with species now living in the Red Sea."

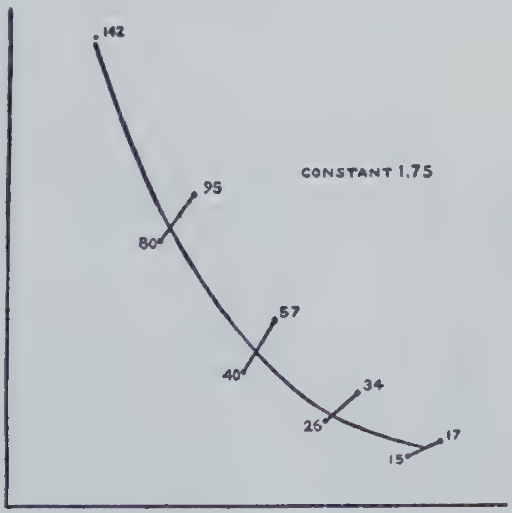

Fig. 3.—River Nive (Pyrenees). *U.G.I.*, i, 9.

In the interior of the Depression terraces below sea-level are to be discerned on all sides ; but measurements are not given with accuracy. The engineer of a mining railway showed a step at -20 to -30 metres and at -50 metres. There was a third step to about -62 metres and measurements to the basal plain of sand, mud, and salt are given at -95 and -114 metres.

Stefanini goes on to say " One might ask, as did Dainelli and Marinelli, if there cannot also be included in the category of terraces those 'marine or lacustrine deposits standing out like castle keeps from the plain' which forms the bottom of the basin and reaches down . . . to a depth of —140 m."

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[1] *U.G.I.*, ii, 2.

Besides the beach, terrace, or erosion platform at heights above sea-level that have been mentioned, there are therefore others at — 50, — 95, — 114, and the bottom of the Depression at —140 metres.

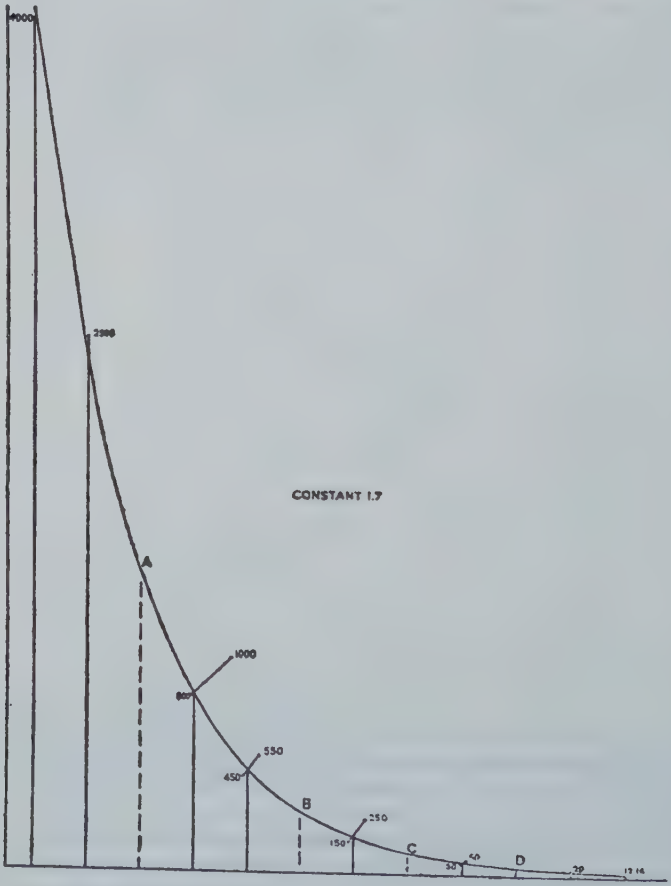

Fig. 4.—South Africa. *U.G.I.*, i, 19.

It is hardly necessary to stress the importance of any features below present sea-level in the search for a reason for rhythmic arrangement. For, if such stand in a definite relationship to those above sea-level, the scope of the search is very much narrowed down.

Patience was necessary in the case of the Dancala Depression; but finally it was found that if 20 metres were added to all the positive beaches and subtracted from all the negative ones, a mathematical relationship could be demonstrated.

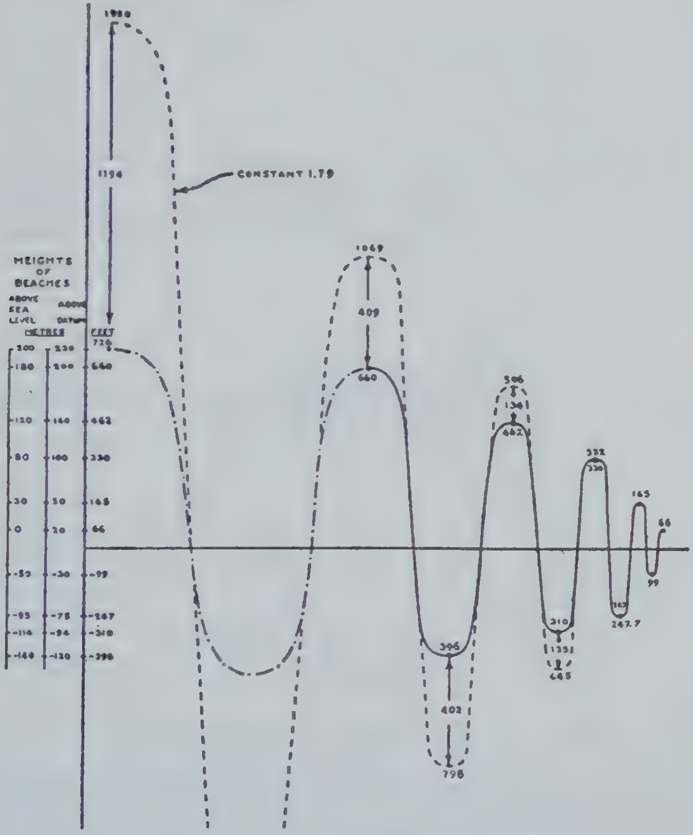

FIG. 5.—Dancala Depression. *U.G.I.*, ii, 2.

Fig. 5 shows the curve that was plotted and also—in broken lines—a theoretical ripple curve in which amplitude and half-wave-length (crest to trough) increase by 1·34, the constant proportion between adjacent amplitudes being 1·79. It is to be pointed out that, while the penultimate oscillation at − 247 to 330 feet agrees with the theoretical, the preceding oscillation is deficient by 134 and 135 feet respectively, and the one before that by 402 and 409 feet; no figure can be given for the first trough, but the first crest is deficient by

1,194 feet. Further, each successive deficiency is almost exactly three times the last. In the face of these coincidences this relationship to a theoretical curve requires a deal of explaining away.

The Dancala Depression assisted the search by giving a faint indication that the rhythm of the changes in base-level may have been a true oscillation, and it is necessary to define the sense in which this word is used in this paper. Elsewhere many authors have used the word oscillation when speaking of marine transgression followed by regression, without reference to the extent of either; and it is not clear whether they have a true oscillation in mind.

A *sustained* or *forced* oscillation, in which a point moves continually between certain positive and negative limits is not here in question. In a *free* oscillation a point moves from a maximum on one side to a less distance on the other, back to a proportionally less distance, and so on until it comes to rest on the axis of oscillation.

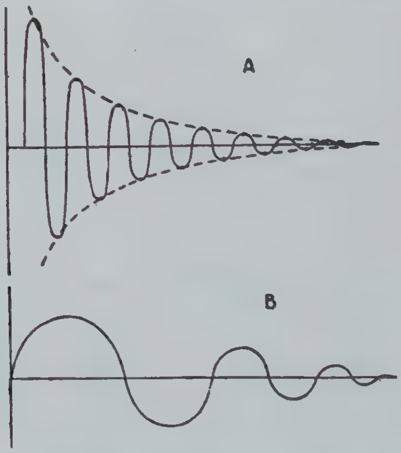

Fig. 6.—A, Free Oscillation. B, Ripple Oscillation.

If this motion be plotted as a series of waves of diminishing amplitude but equal wave-length, points at the crests of waves will lie on a decremental curve and points at the troughs on another, inverted, as Fig. 6A. In a *ripple* oscillation, amplitude, and wave-length mutually decrease, as Fig. 6B.

The characteristic shape of the curves in Figs. 3 and 4, especially with the submarine features noted in respect of the latter, support the above faint indication that the rhythm may have been a true oscillation, though it is not possible, without some indication as to wave-length, to determine whether a free or a ripple oscillation is in question. So far, too, it is not apparent whether the earth's crust has moved or whether we have to do with a true oscillation of sea-level.

Heights as given	Nearest common proportion	Ditto metres	Intervals	Heights, common basis	Heights as given	Nearest common proportion	Ditto metres	Intervals	Heights, common basis	Heights as given, metres	Nearest common proportion	Intervals	Heights, common basis	Heights as given, metres	Nearest common proportion	Intervals	Heights, common basis	Heights as given, metres	Nearest common proportion	Intervals	Heights, common basis	Heights as given, metres	Nearest common proportion	Intervals	Heights, common basis
				+ 6·2					+ 12·4				+ 3				+ 4				− 1·6				+ 6
50 to 60	59	17·8	12·5	24	35	38·2	11·6	12·1	24		21	14·4	24	20	20	14·2	24		25·6	15·4	24	15 to 18	18	13·5	24
	100	30·3	21·2	36·5	75	78·3	23·7	24·9	36·1	30 to 45	35·4	24·1	38·4	32 to 35	34·2	24·2	38·4	40	41	24·6	39·4	30	31·5	23·6	37·5
150 to 250	170	51·5	36·1	57·7	175	160·5	48·6	51·1	61	60	59·5	40·5	62·5		58·4	41·6	62·4	65	65·6	40·4	64	60	55·1	41·3	61·1
	289	87·6	61·4	93·8	325	329	99·7		112·1	90 to 110	100	68	103	100	100		104		106	63	104·4	100 to 120	96·4		102·4
450 to 550	491	149		155						150 to 230	168		171					150	169		167				
S. Africa *U.G.I.*, i, 19 c = 1·7					Sierra Leone *U.G.I.*, i, 18 c = 2·05					Quadalquivir *U.G.I.*, i, 51 c = 1·68				Ebro *U.G.I.*, i, 51 c = 1·71				Bilbao *U.G.I.*, ii, 75 c = 1·6				Mekong *U.G.I.*, i, 17 c = 1·75			

Depéret, in whose view changes of sea-level were responsible for the presence of raised beaches—the land remaining steady during the time of their formation—attempted some correlation of beaches occurring at similar heights above sea-level. The very much more varied data now available show that both positive and negative movements of the land can be demonstrated in many places, so that similar height above sea-level is no criterion of age, of itself.

Of the communications to the first volume of the Report of the Commission on Pliocene and Pleistocene Terraces, one of the most exhaustive is Chaput's on the Rivers and Seaboards of France. One fact emerges with unmistakable plainness, that all the coast from Belgium round to the Pyrenees has been slowly sinking since a time which seems to connect with the later glacial period. A deposit of alluvium has kept place with the sinking, so that large tracts are at about sea-level. This deposit was called Flandrian by Dubois, who distinguished (*a*) a lower Flandrian with mammoth and Corbicula shells, (*b*) a neolithic, and (*c*) a more recent phase, during each of which minor oscillations could be detected.

In Brittany there are no plains of recent alluvium ; but submersion is evident as megalithic monuments are below low-water mark and Brest harbour is a drowned valley ; also at Morbihan Neolithic implements are found 6 metres below low-water mark. The underlying Cretaceous or Tertiary deposits are now at — 20 to — 30 metres and seem to represent an erosion platform.

Having these movements in mind, and also similar or opposite movements elsewhere, it will obviously be safer to correlate beaches which are present in sets not by their height above sea-level, but by the intervals between beaches. In Table I the corresponding beaches at several different sites have been set out side by side, the nearest constant proportion between their heights being given at the bottom, the average constant of those given in the table being 1·75. If curves[1] are plotted for all these sites, the curves have the characteristic shape of those in Figs 3 and 4. The lower part of the Bilbao series has been included because, although this site shows a falling-off when the larger movements are concerned, the smaller movements are nearly in that proportion which may be considered characteristic of the more stable regions.

An examination of the intervals given in Table I shows that there is an interval of the order of 23 metres followed by another of the order of 41, followed by another of the order of 63 metres, and these intervals should be sufficient to establish for any site whether it has moved up or down since or during the formation of the beaches.

Following the faint indications noted above—that the rhythm may have been in the nature of a true oscillation—it was necessary to examine current theories in order to see which fitted in best with these new observations, and also to endeavour to explain the important difference in behaviour as between Figs. 1 and 2 and Figs. 3 and 4. It was necessary to consider whether the movements were eustatic, that is of the sea relatively to the land, or epeirogenic, that is of the land relatively to sea-level.

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[1] With equally spaced abscissæ these are logarithmic curves, and the constant proportion is usually referred to as the exponential.

Eustatic theories depend upon the incontrovertible fact that locking up quantities of water in the shape of ice-fields means lowering the general level of the sea. Steers[1] has discussed the eustatic hypothesis very fully, and notes such serious objections as the lack of correlation in heights of beaches in different parts of the world; that the faunal indications of the beaches are not all towards warmth, as might be expected at periods of ice melting; and others. Nor is any precise rhythm to be detected in the glaciations.

Except, perhaps, in the case of Daly's "Glacial Control Theory",[2] advocates of the eustatic theory invoke isostatic or tectonic movements of the crust to assist in explaining phenomena: W. B. Wright in particular[3] has shown how the Yoldia and Littorina beaches might be accounted for by an isostatic recovery of the land from depression under the weight of ice, combined with higher sea-level as the icefields melted. Isostatic recovery, however, does not explain the Baltic movements of the preceding Ancylus period, the land sinking in the north and rising in the south, so that the waters of the Ancylus lake retreated farther and farther north.[4] Also the date of the Yoldia Sea is given by Brooks as about 8000 B.C.,[5] at which time most of the icefields—at least in the northern hemisphere—had already melted.

Eustatic theories alone cannot account for beaches at great heights above sea-level; nor are their advocates agreed in their estimates of lower sea-level due to the glaciations.

Referring to the curves in Figs. 1 and 3, it is difficult to imagine a combination of varying sea-level and simple tectonic movement which would explain the difference in behaviour at these two sites, which are open to the same ocean, one at each side of the Pyrenees.

It would be idle to deny that the locking up of water during the glaciations had any effect; but it was concluded from the foregoing that such effect was small, and that most of the movement observed was due to rise and fall of the land relatively to the sea.

As to the difference in behaviour between the sites in Figs. 1 and 2, on the one hand, and those in Figs. 3 and 4, on the other, it was argued from the indications in Fig. 5 that an oscillation of the earth's crust originated from somewhere below it *or* that an oscillation of the upper crustal layers originated from below them, and failed of its full effect in the case of the first group by reason of strain in an intervening layer.[1]

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[1] *The Unstable Earth.* London, 1932.

[2] *Proc. Amer. Academy Arts and Sc.*, li, 1915.

[3] *Quaternary Ice Age.*

[4] Brooks, *Evolution of Climate*, 1922, p. 121.

[5] *Ency. Brit.*, 1929, "Climatic History."

Further, if the cause of the rise and fall took the shape of waves or moving-anticlines, the observed strain may have been due to the flow of plastic material from its position on a crest towards the trough on each side.

Gilbert [2] has adduced evidence for what resembles a true oscillation in the south of England from late glacial times onwards; and it will be remembered that the Flandrian deposit contains evidence of oscillations independent of the general sinking of the land.

It was therefore concluded that the search pursued two different directions; in other words that a protracted true oscillation might have been responsible for the formation of those raised beaches which show rhythm—the beaches being formed at points of little or no motion corresponding to the troughs of the oscillation—but that an independent mechanism was responsible for other movements up and down. This independent rise or fall, it was considered, would affect the constant at any given site, for if land were slowly rising or slowly sinking while a series of beaches were being formed, the figure would be increased or decreased because each beach would be rather further from the next below, or nearer to it. The alternative was not held to be conceivable, that is that within a circumscribed area the magma—presumably the vehicle of the oscillation—could oscillate at different rates.

The next step was to look round for a possible cause for such an oscillation. The beaches are generally agreed to spread from the beginning of the Quaternary, or perhaps slightly earlier, over most of the Quaternary. Therefore any originating cause must be sought for in the Tertiary, preferably middle or late. The outstanding event of the Tertiary was the elevation of the Alpine mountain systems; and it was considered that the concentration of so much mass might be the cause sought for. It was therefore necessary to investigate the possibility of an oscillation arising as a consequence of mountain-building.

The doctrine of Isostasy states " that wherever equilibrium exists on the earth's surface, equal mass must underlie equal surface areas ".[3] This means that the outer crust of the earth is " floating " on a denser substratum, and that, where an upward protuberance is found, such as a mountain chain, there is below it a downward protuberance of the lighter material into the substratum.

There is evidence pointing to the existence of several shells of increasing density within the crust, the layers below the upper sedimentary layer being the Granitic, Intermediate (tachylyte or diorite) and Lower Layers (dunite, peridotite or eclogite), according to Jeffreys.[1] On the concentration of extra mass on top this " would force the crust down, the fluid below would flow out and the process would continue until just enough had flowed out to restore the balance ".[2] The depth at which the outflow takes place was calculated by Heiskanen to be 40–80 km.; but Jeffreys says: " The elastic bending of the upper layer will spread the compensation a little, and, when allowance is made for this, the most probable thickness is only about 30 kilometres." [3]

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[1] Professor Ewing (*Strength of Materials*, 1914) conducted some experiments in which a load of amount exceeding the elastic limit was slowly applied in tensile tests of soft iron wire. The stress-strain diagram showing the plastic strain effect is strikingly similar to Figs. 1 and 2.

[2] *U.G.I.*, ii, 11.

[3] Steers, *The Unstable Earth*.

Such a flow of rock-material must continue spreading out on the surface of the spheroid of magma until the displaced material is incorporated into the general spheroid. There is, however, no room for it, the adjacent crust being in contact with the adjacent magma, so that it cannot so flow without raising the adjacent crust. As the flow spreads, therefore, its passage will be marked by a travelling anticline on the surface of the crust, its form possibly being a steep face with a gentle reverse slope. The geographical effect will be a marine regression followed by a return to about the normal.

Such conditions must follow a slow concentration of mass; but it is necessary to inquire whether a rapid concentration would have other consequences. In Fig. 7 it is supposed that concentration of mass takes place at a rapid rate (*a*, *b*) before the outflow of rock material can begin. The downward protuberance for compensation would then take place in such a manner as to increase the height of the anticlines of outflow. The analogy of a buoyant object thrown into water will not be missed; but it is questionable whether magma is fluid enough to allow of this kind of oscillation. In view of the undeniably high viscosity of these layers, such a thing would not seem mathematically possible. Yet the beaches seem to be linked in some way with mountain-building.

In any case, it was considered that rapid concentration of mass would be followed not by a gentle outflow but by a complex disturbance of the adjoining magma which might well take a rhythmic form. Attention was directed to the Somme and Thames terraces, as such rhythmic disturbance must be reflected in the regimen of mature rivers in the vicinity.

In England Dewey [4] notices upland tracts at about 400 feet (123 metres) and lower plains at 300 and 200 feet (90 and 60 metres), but he ascribes these latter to the result of sub-aerial agencies. Below the 200 ft. plain the terraces associated with the existing rivers come on; in the case of the Thames these are usually known as the 100 ft. (30 m.) or Boyn Hill, 50 ft. (15 m.) or Taplow, and 20 ft. (6 m.) terraces, and there is a buried channel the depth of which is usually given as 70 feet, though at the mouth of the present river it is upwards of 120 feet below river level.

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[1] *The Earth*, 1929.

[2] H. Jeffreys, *Ency. Brit.*, 1929, "The Earth."

[3] Ibid.

[4] *U.G.I.*, i, 10.

Around Oxford the 100 ft. terrace is called the Handborough Terrace. At 40–50 feet above the present river is the Wolvercote Terrace, which is not always clearly differentiated. Associated with it are the deposits of the Wolvercote Channel and the Warp, which is a deposit indicating a cold, wet climate : it rests on the deposits of the Wolvercote Channel, i.e. about 55–60 feet above the present river. The next, or 20 ft. terrace, is the Summertown-Radley Terrace, which is the most extensive, and is subdivided into the lower and older gravels, and the upper and newer gravels. Below this, at 10 feet, are the flood-plain gravels. A buried channel is also present.[1]

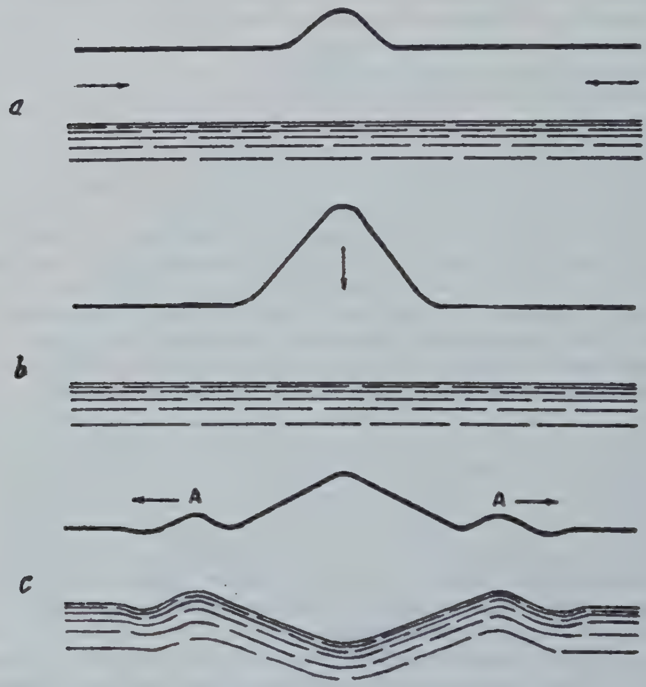

Fig. 7.—*a*, Mountain-building proceeding under lateral pressure. *b*, Commencement of movement to establish isostatic equilibrium. *c*, Equilibrium established : rock-material flowing outwards causing anticlines A.A at the surface.

A good deal of difficulty has been experienced in attempting to date these terraces. Some of the warring considerations are as follows: Gregory[1] gives ground for supposing that the 100 ft. beach in Scotland was formed after the last Ice maximum (Riss ?). Both Wright and Marr give ground for thinking that the 100 ft. terrace corresponds to the depression which formed the Yoldia Sea in the Baltic region, a date for which is suggested by Brooks[2] as 8000 B.C., that is, in times which were Transitional in France, and this is impossibly late for the Thames Valley 100 ft. Terrace, as Dewey[3] shows that the depression here connects with Mousterian times. The French terraces were formed *before* the end of the last phase of glaciation as, Commont considers according to Burkitt,[4] that the Older Loess was deposited just previous to the last glaciation and the Younger Loess just subsequently.

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[1] Sandford, "The Geology of the Country Round Oxford," *Mem. Geol. Surv. England*, 1926.

The key to the puzzle may be given by the signs of duplication, mentioned by Burkitt, in more than one of the terraces. Similarly, two of the terraces at Oxford are seen to be duplicate. Also, Dewey mentions that the Boyn Hill terrace of the Thames is really double and, after speaking of the simple character of the implements on the high terrace and showing that Le Moustier man was afterwards in occupation of a shore at about the present level, goes on to say: "Subsequent to this period a cold climate brought about new conditions in which great sludge-streams overwhelmed the valleys and buried the working-floors . . . The climate gradually ameliorated and brick-earth was laid down on what appears to have been sinking land, so that it rises level with, even if it does not actually overlap, the deposits of the 100 ft. terrace."

Hazzeldine Warren, too,[5] remarks on the "two noted deposits of the Grays district . . . Little Thurrock and West Thurrock respectively. These deposits . . . although they both belong to the 50 ft. Terrace . . . the fauna shows to be of different dates". He concludes that there was an important rise of the land—in between the dates of these two deposits—when the channel of the Thames ran by Clacton.

With the faunal differences in mind, together with all the evidence of duplication, the question of these terraces was reconsidered, together with theories as to why they should be covered with gravel. As to the latter, those who do not rely on earth movements consider that the effect of glacial periods was to absorb moisture from the air, causing streams to dwindle. In interglacial times the river beds were filled by overladen streams, the terrace-gravels being deposited as a result of flooding. Against this, there is a good deal of evidence of earth movement: some of the faunal evidence is of warmth:

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[1] *U.G.I.*, ii, 10.

[2] *Ency. Brit.*, 1929, "Climatic History."

[3] *U.G.I.*, i, 10.

[4] *Prehistory*.

[5] "The Palaeolithic Industries of the Clacton and Dovercourt Districts," *Essex Naturalist*, xxiv, pt. i.

counting the duplications, there are many more terraces than glacial periods: and other objections. Sollas has recently favoured the suggestion that the river terraces arose in consequence of the advance and retreat of the sea.[1] "Marine shells were long ago recognized by Prestwich as occurring on the second terrace at Menchecourt."[2]

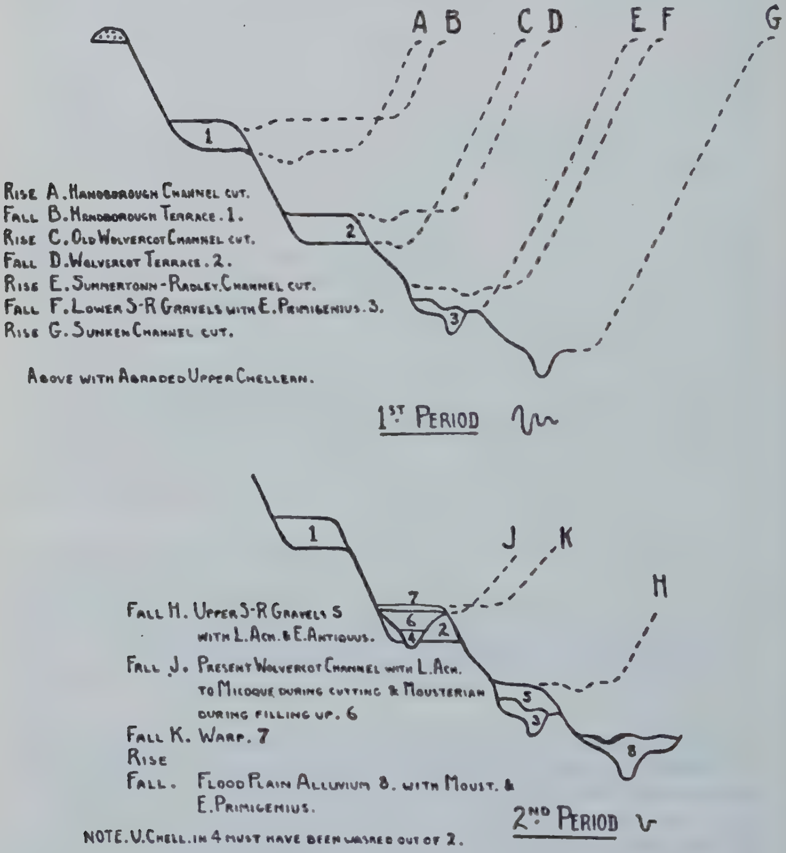
A B C D E F G
1
2
3
Rise A. Handborough Channel cut.
Fall B. Handborough Terrace. 1.
Rise C. Old Wolvercot Channel cut.
Fall D. Wolvercot Terrace. 2.
Rise E. Summertown-Radley. Channel cut.
Fall F. Lower S-R Gravels with E. Primigenius. 3.
Rise G. Sunken Channel cut.
Above with Abraded Upper Chellean.
1st Period
J K H
1 7 6 4 2 5 3 8
Fall H. Upper S-R Gravels 5 with L. Ach. & E. Antiquus.
Fall J. Present Wolvercot Channel with L. Ach. to Micoque during cutting & Mousterian during filling up. 6
Fall K. Warp. 7
Rise
Fall. Flood Plain Alluvium 8. with Moust. & E. Primigenius.
NOTE. U. Chell. in 4 must have been washed out of 2.
2nd Period

Fig. 8.—Oxford (after Sandford).

The faunal indications seem to point to a series of movements occurring before the last phase of glaciation (Mindel-Riss ?) and a second series with which the glacial period closed.

Fig. 8 shows how the movements at Oxford can be analysed into two periods with a definite number of rising and falling movements in each ; the first period a warm one, except that mammoth remains in the lower Summertown-Radley gravels indicate an approaching cold, and the second period late glacial. Fig. 9 shows a similar analysis of the Somme terraces, with exactly the same number of rising and falling movements in the first period, but with an extra

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[1] Geol. Mag., LX, 1923.
[2] Burkitt, *Prehistory*, Cambridge, 1921.

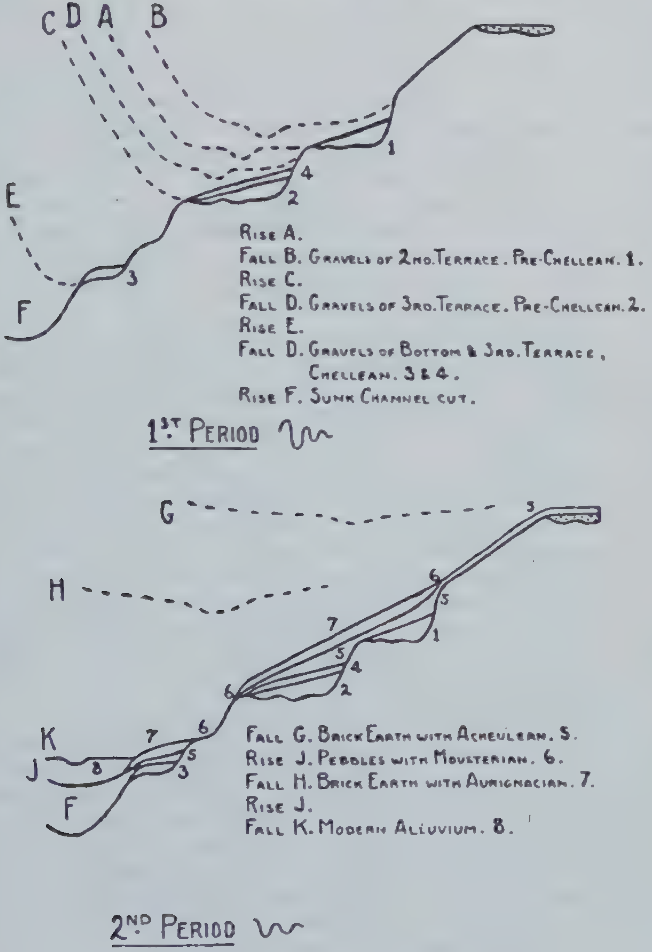
C
D
A
B
E
F
1
2
3
4
Rise A.
Fall B. Gravels of 2nd. Terrace. Pre-Chellean. 1.
Rise C.
Fall D. Gravels of 3rd. Terrace. Pre-Chellean. 2.
Rise E.
Fall D. Gravels of Bottom & 3rd. Terrace, Chellean. 3 & 4.
Rise F. Sunk Channel cut.
1st. Period
G
H
K
J
F
1
2
3
4
5
6
7
8
Fall G. Brick Earth with Acheulean. 5.
Rise J. Pebbles with Mousterian. 6.
Fall H. Brick Earth with Aurignacian. 7.
Rise J.
Fall K. Modern Alluvium. 8.
2nd. Period

Fig. 9.—Somme (after Commont).

rise and fall in the second period connecting with Aurignacian times. The two brick-earths here are the Older and Younger Loess, but we have seen above that Dewey considers the first brick-earth on the Thames was water-deposited. Other authorities have questioned whether the Loesses are true loess; but the point has little importance for the present purpose.

Taken by themselves, the Thames terraces can be referred to an oscillation the waves of which increase in amplitude, crest to trough, by 33 per cent, giving a constant proportion between adjacent waves—crest to crest—of about 1·77. The axis of the oscillation, representing old normal sea-level, would lie about 18 feet below present level, which makes the levels read (instead of 20, 50, and 100) 38·4, 68, and 118 feet, with the buried channel 51 feet below the axis. Fig. 10, in order the better to show how the terraces could have been formed, has been drawn to show the water-level rising and falling; but it is to be understood that the movements were of the land relative to a more or less stationary sea-level. The curve has been carried down to our time to show how its extension corresponds to the rising and falling deduced by Gilbert,[1] which seems to be somewhat of the nature of a true oscillation. The time-scale is, of course, purely tentative and provisional, being erected on only a hypothetical basis.

That the axis should appear below present level is in accordance with the findings of the Ordnance Survey, the south-east of England subsiding relatively to the north-west of the country,[2] though it is doubtful whether the rate of subsidence is as high as 1 foot per century. The heights of the terraces at Oxford above the river then conform to the scheme of a mature river with terraces, the latter gradually nearing the river bed as one proceeds towards the source.

If an oscillation of land-level did occur due to the rapid building of the Alpine system, it would fall mostly in the Quaternary and presumably affect only the adjacent regions such as Europe and North Africa. The dating does not fit for the South African beaches, as Krige states that some are covered with a veneer of Mio-Pliocene sediments: also for the Dancala Depression levels no dating is given except as to the recent beach now at sea-level.

To avoid misconception, it is desirable to state that, as an oscillation arising at some considerable depth in the crust has been imagined, the impulses are not necessarily supposed to have arrived at the surface in a recognizable form: not only is there reason to suspect the presence of an intervening layer occasionally in a plastic state—sufficient to cause deformation of the waves—but the continual bending of the crust in reversed senses would not be successfully accomplished, with consequent distortion; so that instead of a smooth rise and fall there may have been long standstill periods while stresses were accumulating.

A mechanism which is possibly responsible for one class of movement at the earth's surface having been partly worked out, attention was transferred to another class of movement which shows itself

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[1] *U.G.I.*, ii, 11.

[2] Professional paper No. 14, Brit. Assoc. Meeting, York, 1932.

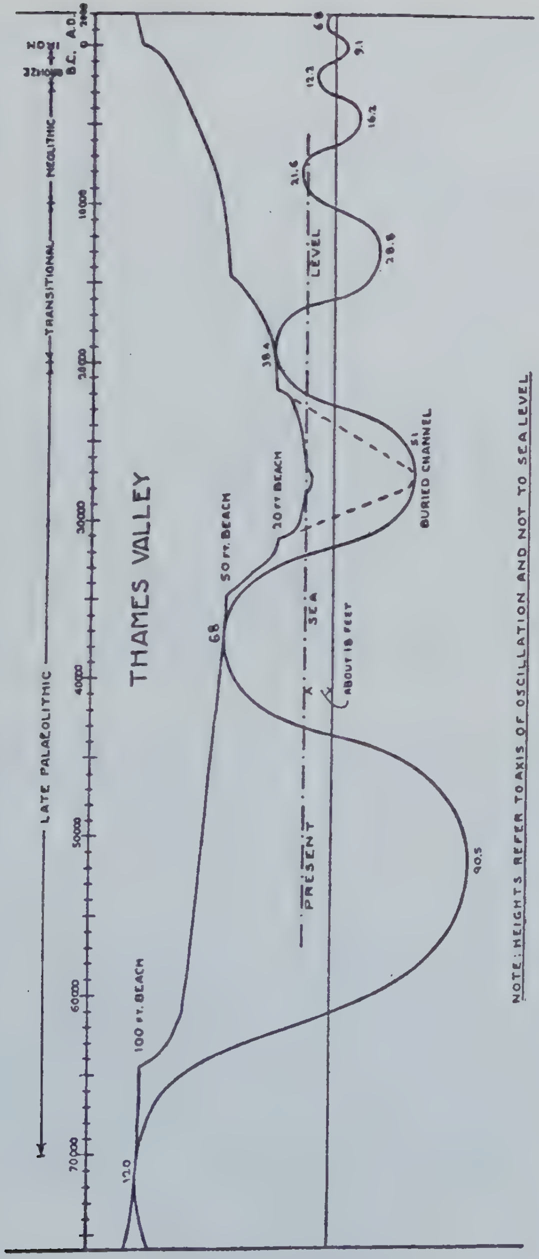

Fig. 10.—Thames Valley.

as a protracted subsidence; or as elevation of one region accompanied by subsidence of an adjoining region. In this case there is no question of rhythm.

It was concluded above that the difference between the two classes of behaviour noticed was due to the presence in one class of a layer not far below the surface in a state of extreme plasticity. During an attempt at the correlation of beaches at the sites round the Bay of Biscay, curious behaviour of France in relation to Spain towards the end of the beach-forming period became evident, which seemed to warrant the deduction that flow occurs in such a plastic layer.

The sites concerned are Bilbao, the River Nive (Pyrenees), and the Armorican massif. On a preliminary examination of the Nive figures[1] one is struck by the wide limits given for certain terraces. The heights are given as 15–17 metres; 26–34 metres; 40–57 metres; 80–95 metres; and 142 metres. In the case of Armorica, on the other hand,[2] the salient peculiarity is the large number of levels and the small intervals between them. The heights given are 128, 90, 65, 51, 42, and, with reserve, 25 and 10 metres.

As regards Spain, E. Hernandez-Pacheco emphasized the sinking of the whole north of Spain,[3] and it is desirable to point out that this sinking seems to have been completed prior to the beach-forming period, as a complete set of six terraces (less one at 106 metres which seems to have disappeared completely) is present at the Bilbao site in its proper position in relation to sea-level. France on the other hand—at least its western coast, which is now being considered—has slowly subsided since late-glacial times.

For correlation, therefore, it was considered that a certain addition should be made to all the Armorican beaches. From Chaput's data, a figure of 20 metres would appear to be indicated; but it will be seen from Table II that only 15 is necessary. Table II shows how, after the addition of 15 metres, some of the Armorican levels can be correlated with Bilbao and others with the Nive.

The land being considered, as stated above, to rise and fall, France does not seem to have reached in any case the same level as Spain; but to have behaved as if a substratum had an elastic quality. It did not sink completely to the 106 level, for instance, but only reached 95 in the case of the Nive and, instead of a terrace being formed at this height, gravels were deposited as the land rose slightly at all heights between 95 and 80 when equilibrium seems to have been reached. Similarly, the terrace which should appear at 66 metres is spread over the distance between 57 and 40 metres, and so on. Taking the last column in Table 2 the French coast was at first consistently 26 metres higher, but this figure fell to 15 and then 10 metres and is now −15 or more.

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[1] *U.G.I.*, i, 92.

[2] p. 79.

[3] *U.G.I.*, ii, 5.

To explain this behaviour, it was considered that between the Pyrenees and Brittany the land was, towards the end of the beach-forming period, raised by a plastic anticline due to the squeezing out of plastic material by the collapse of the floor of the Bay of Biscay: the passing on of the anticline has since allowed the coast to sink first to its former level and then below it.

TABLE 2.—CORRELATION OF BAY OF BISCAY SITES.

Armorica						Nive and Armorica				
Heights as given	15 metres added	Bilbao	River Nive	Apparent succession of levels in Bay of Biscay area	Intervals between levels	Level reached	Metres less than expected	Final level	Further difference	Total less than expected
128·	143	(169)	142	169		143	26			26
					63					
90	105	(106)	95 to 80	106		95	11	80	15	26
					40					
65	80									
51	66	65·6		66		57	9	40	17	26
					25					
42	57		57 to 40							
25	40	41	34 to 26	41		34	7	26	8	15
					16					
10	25		17 to 15	25		17	8	15	2	10

The general acceptance of the doctrine of isostasy has made it difficult to explain the disappearance of areas of land, for, if they were isostatically supported, how could they sink? But that areas have subsided is the conclusion reached after examination of physiographical features at certain places such as the prolongation of the Adour estuary across the bed of the Bay of Biscay and the submarine prolongation of the estuaries of the Congo and St. Lawrence Rivers; also there is evidence of subaerial denudation on the Dolphin Ridge in the vicinity of the Azores.

Holmes considers that disappearance of land areas may be due to a change of condition in one of the crustal layers, one substance changing into another of greater density. This action, however, does not seem to provide an explanation of those movements in which an elevation of one region accompanies a subsidence in another.

Flow in a plastic layer, on the other hand, would account satisfactorily for many earth movements. Search for a mechanism of this nature was therefore undertaken.

Given a plastic anticline in motion, it is not difficult to see that it would continue to move. Fig. 11 shows how the crust—considered

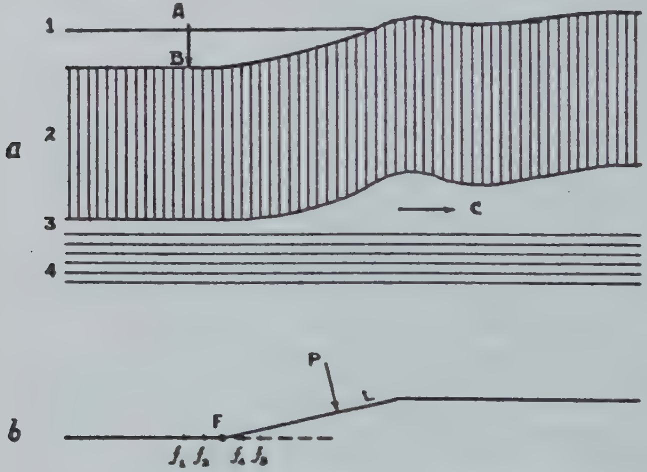

Fig. 11.—Plastic moving anticline.

(*a*).—1 : Sea-level. 2 : Upper crustal layers. 3 : Layer in a plastic state. 4 : Underlying layers. C : Direction of travel of plastic material.

(*b*).—F : Position at any instant of fulcrum of level L. *f*1, *f*2 : Past positions of fulcrum of levers L1, L2 (not shown). *f*4, *f*5 : Future positions of fulcrum of levers L4, L5 (not shown).

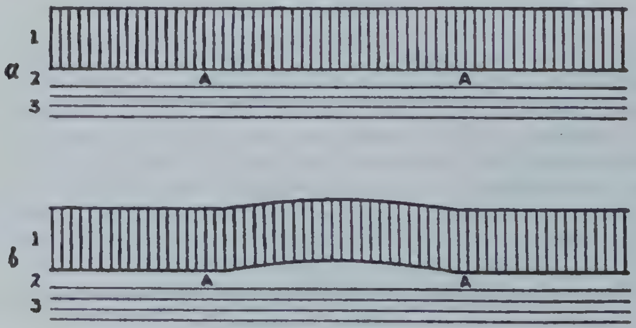

Fig. 12.—1 : Upper sial rocks. 2 : Layer (*a*) about to liquefy between points A, A (*b*) after liquefaction. 3 : Layer next below.

to behave in an elastic manner—in descending through a distance equal to A–B does a certain amount of work which forces the anticline along, its temperature being maintained and increased by friction so that the adjacent material is reduced to plasticity and the action continued further and further to the right of the diagram.

It is more difficult to imagine how such an anticline could be set in motion.

Joly contended that all the heat which is escaping from the surface of the earth can be more than supplied by the radioactivity of the sial rocks, and showed that, if these have an average thickness of 30 kilometres, the temperature at their base must be about 1050° C., or nearly the melting-point of basalt (1150° C.). He calculated that eventually sufficient heat would accumulate to reduce the layer to a molten condition.[1]

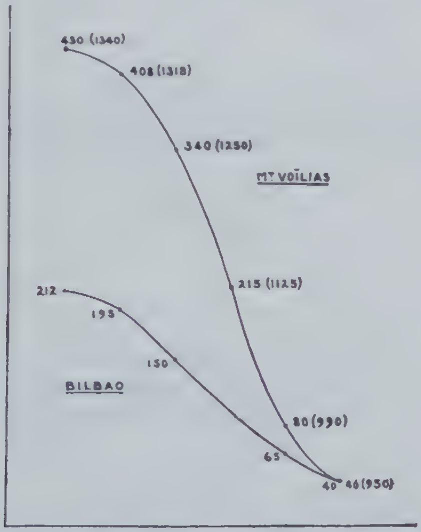

Fig. 13.—Heights of raised beaches at Bilbao compared with those at Mt. Voïlias.

Fig. 12*a* shows in diagrammatic form a featureless section of crust. It is to be supposed that heat has accumulated until a portion of the unshaded layer is beginning to liquefy. This means that expansion takes place, producing stresses which delay the liquefaction until suddenly the upper layers give way and, the stress being relieved, liquefaction is rapid. 12*b* shows the dome-like blister produced. This form is not unknown: there is tension in the upper crustal layers resulting in manifestations of vulcanicity and much faulting. Examples are Central Africa and the district around the Gulf of Corinth: the former was raised into a dome during the Cainozoic and faulting has produced the well-known rift-valleys, some of which are occupied by lakes such as Tanganyika.[1] In Greece, the Pliocene formations of the Gulf of Corinth have been raised to 1,759 metres,[2] and earthquakes in this and the adjoining regions are frequent. Fig. 13 is interesting because it shows how during the beach-forming period this part of Greece was steadily rising and also that this rise has continued since, for the beach that appears at 40 metres at Bilbao is at 950 metres here.

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[1] See especially *The Surface History of the Earth.*

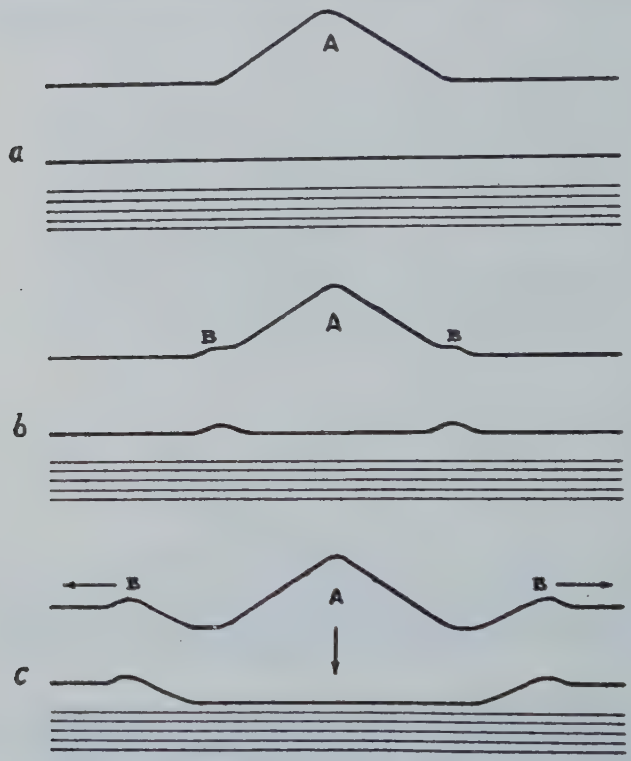

Fig. 14.—(*a*) Layer about to liquefy beneath elevation A. (*b*) Expansion has raised anticlines at B, B. (*c*) Collapse of A has initiated moving anticlines B, B.

There is here no reason to be seen why any plastic anticline should be set in motion. Fig. 14, however, shows another case where this might conceivably occur.

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[1] *Ency. Brit.*, 1929, "Africa, Geography."
[2] Philippson, *der Peloponnes*, p. 426.

It may be assumed that the molten state would be reached sooner under a mountain range, the greater thickness of the sedimentary rocks here acting as a blanket in reducing the rate of heat-flow to the outer air. The mass above is greater than in Fig. 12, and the weakest places, at which the giving-way would occur are at B, B. Any cantilevering effect from right and left of the diagram is destroyed and the median mass A is only supported vertically. The plastic layer having rapidly liquefied below B, B, the central part is unable to support the mass and is squeezed out to right and left. 14*c* shows the two plastic anticlines commencing to travel away from the median mass in the manner of Fig. 11.

The plastic anticline, therefore, the passing of which raised France towards the end of the beach-forming period and has since allowed it gradually to descend, may have originated as one of a pair. The rate of travel must be extremely slow, for Chaput shows that the descent has been only about 20 metres since Würmian times, in the case of the north and west coasts of France.

SUMMARY.

There is reason to suppose that a rise and fall of any part of the earth's surface may be due to anticlines travelling through the crust either (*a*) at the depth at which isostatic compensation becomes effective, which may be likened to the "surface" on which the crust "floats", or (*b*) at a less depth, in a layer which becomes molten through the action of radioactivity.

In the case of (*a*) the cause would seem to be mountain-building in the crust above and the anticlines are outflows of rock-material: the rise and fall may be about equal: there is also some slight reason to suppose that an oscillation, analogous to that occurring when a buoyant object is thrown into water, with the production of a series of waves of diminishing amplitude, has occurred, but it is not yet clear whether such an oscillation is possible.

In the case of (*b*) the cause of the anticlines is the collapse of a portion of the crust which squeezes out plastic material below: a small rise is followed by a long, slow fall: a rise in one region is accompanied by a fall in an adjacent region: areas may be raised into domes by collapse of surrounding areas, in which case the central area will be a seat of vulcanicity.

REVIEWS.

Igneous Rocks and the Depths of the Earth. By R. A. Daly. pp. xvi + 598, with a plate and 190 figs. New York and London: McGraw-Hill Book Co., Inc., 1933.

In a certain sense this book may be regarded as a new edition of the author's well-known work "Igneous Rocks and their Origins" published in 1914. But, as is duly pointed out in the preface, it has undergone changes so great that it must in reality be regarded as a new work. The difference is actually very largely expressed in the change of title. In the present book far more attention is paid to recent developments of knowledge of the conditions prevailing in the interior of the earth, so far as these are accessible to observation and inference, and here it has to be admitted that a mighty structure of mathematical and physical inference has been built up on the interpretation of earthquake phenomena.

The simplest way to explain the difference between this book and that of 1914 is to copy the paragraph in the preface in which the author himself summarizes the changes: it could not possibly be done more lucidly or more concisely.

"The more vital changes in opinion, argument, and speculative theory include the following: new doubt concerning the relation of the earth's contraction to mountain-building and to the ascent of igneous melts from depth; new emphasis on at least moderate horizontal displacement of large parts of the continental crust over the earth's body; new emphasis on magmatic stoping and the correlated abyssal pure melting of rocks—both on a scale considerably bigger than that imagined in 1914; less emphasis on liquid immiscibility in magmas; more attention to the contrast in the magmatic conditions of early Pre-Cambrian time as against Palaeozoic and later time, illustrating once again the mistake of accepting uniformitarianism as an infallible guide in geology; new stressing of the fact that no one process can explain the diversity of the igneous rocks, especially the multitude of alkaline types."

As an addition to this admirable summary we find also, as might be naturally expected, throughout the book a very strong advocacy of the author's well-known conception of the vitreous basaltic substratum, which in fact may be considered as a lubricant, facilitating the postulated movements of the solid upper layers of the crust; for this is really what the argument as to the physical properties of this layer comes to. It is supposed to behave as an elastic solid when subjected to low pressures of brief duration, then possessing marked rigidity, but at the same time it has nearly or quite zero strength, yielding indefinitely before small shearing stresses and thus behaving like a viscous liquid. In fact it is supposed to be a viscous liquid as hard as steel. Quite a lot of odd things can be done with a substance having such properties.

It is perhaps hardly necessary to refer in detail to all the points raised in the extract quoted above. The perennial subject of the earth's contraction is not discussed at length in the book, and in fact it must be regarded as now *sub judice*, and in need of more facts of observation before an opinion can be pronounced. With regard to horizontal displacement of continental blocks also judgment should be suspended. There are many facts of palaeontology and palaeobotany which are difficult and, as some high authorities venture to say, even impossible to explain unless land-masses have moved long distances, whereas physicists and mathematicians maintain that such movements on the required scale are impossible. Professor Daly's demands here are not great, and would probably not satisfy the biological geologists. Nevertheless he clearly attaches great importance to the observational fact that at one and the same time in earth history effects of tension, as shown by faulting and diking, have occurred in one part of the world, and orogenic uplift with compression and overthrusting in another part. It is difficult to see how this can be explained without bodily lateral transport of crust blocks.

No geologist who is acquainted ever so slightly with one of the great early Pre-Cambrian areas of the world can doubt that Professor Daly is entirely right in his contention that conditions in that early period were fundamentally different from those that prevailed in later ages. The comprehensive study of the Bushveld Complex recently published by Dr. Hall is in itself sufficient to show that even up to fairly late Pre-Cambrian times things happened that are literally inconceivable in modern geology: and this is only one out of many instances. It is interesting to contemplate the possible reactions of the Geological Society of London, for example, to an area as large as Scotland suddenly raised to a red heat. But such awkward incidents do not now occur, and the Society may hope to pursue the even tenour of its way in peace, undisturbed by petrogenetic episodes on the grand scale. All the same, however, there seems to be no particular reason why a phase of basaltic fissure eruptions should not take place anywhere and at any time. It might be well worth while to investigate why the Tertiary igneous activity of Scotland localized itself just *where* it did: the *when* seems to be readily explicable from Alpine orogenesis.

To deal very briefly with one of the other points specially emphasized by Professor Daly, most petrologists would probably agree that there is little evidence of the formation of two consolute silica liquids from one magma, while it is quite clear that the separation of sulphides from silicates is effected by their limited mutual solubility in the liquid state, which is an experimental fact and not a theory.

It is obviously out of the question in a review to enter into the interminable subject of differentiation and the origin of the alkaline rocks, but it is impossible not to agree heartily with the last sentence of the paragraph quoted above. It might be wished that all petrologists would be equally candid, but it seems to be difficult to grasp the idea that analogous results may be produced by a variety of different processes.

Whether one agrees with all his views or not, and the dominating idea of the vitreous basaltic shell seems to be specially open to question, it has to be admitted that Professor Daly has produced a masterly work, setting forth all the facts, discussing them in a temperate and persuasive manner, with a charming literary style and delightful absence of dogmatism. Few, if any, geologists know the facts so well, and therefore the theoretical and speculative views founded on them must receive the most careful consideration.

R. H. R.

ON THE MINERALOGY OF SEDIMENTARY ROCKS. A series of Essays and a Bibliography. By P. G. H. BOSWELL, O.B.E., A.R.C.S., D.I.C., D.Sc., F.R.S., Sec.G.S. pp. vii + 391. London: Murby, 1933. Price 21*s*.

THIS book is divided into two nearly equal parts, of entirely different character: the second half, which is most conveniently dealt with first, consists entirely of abstracts of the vast literature of sedimentary petrology, excluding textbooks. This, of course, will be of very great value to all workers, but does not readily lend itself to review or criticism. It is perhaps noteworthy that Sorby's two great Presidential Addresses to the Geological Society are not included, although Bonney's address to Section C at Birmingham in 1886 is very briefly mentioned.

It is not very clear why the subdivisions of the first half of the book are called "essays" on the title-page, because they all deal with various aspects of the same subject. The introduction deals with lists of minerals and methods, while the rest are nearly all sketches of the historical development of sedimentary petrology from different points of view. Although it is true that the study of sediments does depend very largely on the determination of their mineral constituents, yet there are many other angles from which it can be regarded, as is indeed excellently set forth in this book. It may be doubted, therefore, whether "Mineralogy" is the happiest word to use in the title. Petrography or petrology would have been better: the last for preference. Any average reader of a bookseller's catalogue would be apt to think the scope of the book to be less wide than it really is.

Sedimentary petrology is in the main a new science and the great majority of the people who have played the principal part in its development are happily still alive. Now a historical retrospect of the work of living people is a delicate task, requiring a very nice sense of proportion and abundant tact, for obvious reasons. Even men of science are but human and it is impossible that some at any rate will not think that their work has been passed over somewhat lightly, while that of others has perhaps been unduly appreciated. Consequently, we feel that Professor Boswell would have been better advised if he had confined himself to the publication of his admirable bibliography, without entering upon the thorny path of the appraisement of contemporary work, even in the subject in which he is so competent an authority. And here another and rather sordid consideration enters : if a book is half the size it can be published at something like half the price, and under present conditions many people hesitate to pay a guinea for a book, whereas ten shillings is by no means an alarming figure. Frankly, these " essays " make extraordinarily dull reading, and are a distinct handicap on the sales of an otherwise very useful book. The publishers would be well advised to cut them all out and issue the bibliography alone at a reasonable price.

CORRESPONDENCE.

BEACHES IN GOWER.

SIR,—I regret I misunderstood Mr. Baden-Powell's remark concerning the " ecological assemblage " of the Patella Beach of Gower. Not being qualified to express an opinion on the climatic range of lamellibranchs, I defer to his statement that *Ostrea edulis* and *Chlamys varia* are not likely to have lived near the snouts of valley glaciers. Nevertheless, as I pointed out in my letter to the June number of this Magazine, I was concerned to demonstrate that the Patella Beach is pre-glacial only in the sense that it is older than the neighbouring Glacial Drift, which it stratigraphically underlies. There is no information available as to its maximum possible antiquity, but it is at least not younger than the Neritoides Beach.

Mr. Baden-Powell's remarks concerning beach heights are astonishing. In the GEOLOGICAL MAGAZINE for 1927 and 1928, and in his letter to the May number of this year, he repeatedly refers to beaches in terms of their heights. On the other hand, to avoid the obvious confusion consequent upon such a practice, I endowed the Gower beaches with special names ; and in my letter of June I merely quoted him or adopted his terminology when discussing beaches in the manner now condemned by him.

I am sorry Mr. Baden-Powell objects to my calling his post-Glacial beaches of Scotland and elsewhere Neolithic. So far as

I understand the writings of Dr. Wright and others, however, the beach in Scotland, although perhaps not everywhere of the same age, is certainly Neolithic (*sensu lato*). In any case, post-Glacial is very different from Mousterian, and the Patella Beach in Gower antedates even the latter period.

T. Neville George.

University College of Swansea,
Singleton Park, Swansea.
25th September, 1933.

THE LATER STAGES OF THE CYCLE OF EROSION.

Sir,—Mr. C. H. Crickmay's paper on "The Later Stages of the Cycle of Erosion" (Geological Magazine, July, 1933), mentions the scarcity of peneplains in progress of formation.

In Northern Rhodesia, which is essentially a plateau of about 4,000 feet altitude composed of Cambrian (?) sediments intruded by granite, peneplanation is still in progress, rainwash being the agent. Rhodesia is noted for its "dambo" topography; the dambos being treeless swampy stretches, sometimes quite narrow but usually wide near the stream sources. True dambos are seldom seen on main streams but exist on most tributaries and laterite often outcrops on their somewhat rectangular margins. Dambos appear often to follow structural lines and either enter the major streams at right angles or continue straight across them.

The larger streams usually follow the strike and joint systems of the rocks and represent the new system of erosion but dambos on the tributaries are mainly areas of deposition. Slopes are low, and in the rainy season silt is carried to the edge of the dambo, where it is caught by vegetation, the material passing through the bottle-necked outlets being mainly discoloured water.

Slight earthquake shocks are recorded and the plateau uplift is probably still in progress, the strain being taken up on the old fracture lines so often indicated by the dambos. As mountain building ceased in Lower Palaeozoic times and deposits of post-Mesozoic age are not extensive round South Africa, it may be assumed that peneplanation has been in progress in Rhodesia since the Karroo, with only a small proportion of the products of denudation reaching the ocean. The Rhodesian data indicate that peneplanation, contemporaneous with rejuvenation, can take place at moderate altitudes and that rainwash is a simpler explanation than river or marine erosion.

Leslie H. Ower.

The Rhodesian Museum,
P.O. Box 240,
Bulawayo, Southern Rhodesia.
4th September, 1933.

MIGRATION OF DIVIDES.

Sir,—Would it be possible, through the pages of your Magazine, to make an appeal for a critical investigation of a process of which the importance in modern physiographical interpretation requires no emphasis.

This supposed denudational process, known as the migration of divides or headward erosion, is generally assumed to be so potent that it not merely causes an occasional river-capture but brings about a rearrangement of drainage in accordance with geological structure. This rearrangement results from the migration necessarily being greatest where denudation is greatest, namely in soft rocks.

Now it is perfectly clear that if what is claimed for the process is substantially true this differential migration must be as evident in the field as differential denudation itself. It is no less clear that it is needful for physiographers to investigate divides that are suitably situated, or for other reasons thought to have migrated, in order to ascertain the extent of this differential migration in relation to the differential lowering, and so find out in actual fact the conditions in which, and the extent to which, the process acts. It is clear also that until something of this sort is done the extensive literature and elaborate nomenclature based on the unrestricted application of the alleged process scientifically can hardly be regarded as other than supposition.

My own very limited observations show that appreciable migration takes place only where a divide is on the edge of a plateau-peneplain, and then only to a limited extent, conditions not at all capable of general application and quite incapable of causing a rearrangement of drainage. If these observations are found true also in other parts of the world, and by other observers, then all the fanciful nomenclature of streams will have to be discarded as " nonsensequent ".

May I appeal to physiographers to investigate the divides and announce their findings ? So far all that has been obtained from private correspondence is an expression of profound faith and a curiously illogical begging of the question contained in abundant allegations of river-capture, with sometimes even an expression of surprise that any one should venture to question such a well-established theory. Not one of my correspondents has agreed critically to investigate the experiments in migration which Nature has done for us.

E. O. Marks.

Brisbane.
26th July, 1933.

METAGENESIS IN *LEPIDOCYCLINA*.

Sir,—Mr. J. U. Todd, in his interesting paper on " Metagenesis in *Lepidocyclina* from the Eocene of Peru " (Geol. Mag., LXX, 393), maintains that " by Article 2 of the International Rules of Nomenclature the name '*Lepidocyclina r. douvillei* Lisson' cannot be upheld since it could be understood to mean that the form is a sub-species of *Lepidocyclina r.* The name should read either *L. robertdouvillei* or *L. douvillei*, the latter being, however, pre-occupied". He therefore adopts the name *L. peruviana* Cushman. Personally, I should be delighted if Mr. Todd's interpretation of Article 2 of the International Rules of Zoological Nomenclature were correct. It is, however, perfectly obvious that Lisson regarded *r. douvillei* as a single name, and the fact that it is liable to misinterpretation, while exceedingly regrettable, does not contradict anything in Article 2. Undoubtedly Article 2 shares with many other portions of the International Rules that ambiguity and lack of precision which have necessitated the delivery of numerous interpretative opinions by the Commission. So far, however, none of the opinions bearing on this article (there are eight of them) deals with the point raised by Mr. Todd. He would perform a useful service if, instead of taking the matter into his own hands, he were to submit it to the Commission for a definite decision. The address of the Secretary to the Commission is Room 302, U.S. National Museum, Washington, D.C., U.S.A.

F. A. BATHER,
Member of the International Commission on Zoological Nomenclature.

46 MARRYAT ROAD,
WIMBLEDON, S.W. 19.
14th September, 1933.

Note

Professor Dr. O. Abel, of the University of Vienna will give a course of three lectures on "Palaeobiology and Evolution" at University College, Gower Street, W.C. 1, on 24th, 27th, and 29th November, 1933, at 5.30 p.m. Admission free, without ticket.